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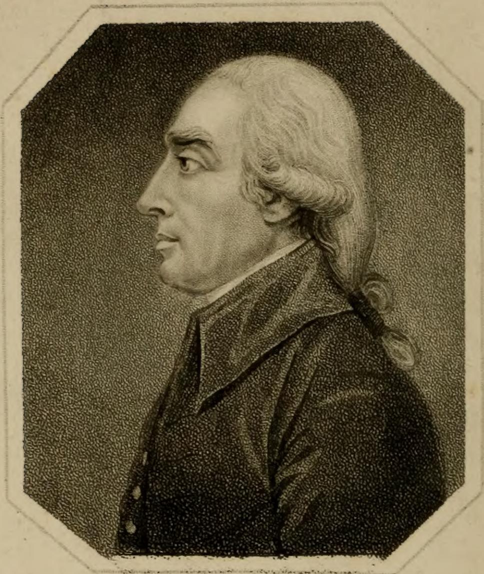

From Tassie. Mackenzie sc.

Joseph Black M.D

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THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, &c.

"Nec aranearum ſane textus ideo melior, quia ex ſe fila gignunt. Nec noſter vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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CONTENTS

OF THE

FIFTH VOLUME.

 Process

Account

THE

PHILOSOPHICAL MAGAZINE.

OCTOBER 1799.

I. *Obſervations on Animal Electricity, and particularly that called Spontaneous. By* J. J. HEMMER *.

UNDER Animal Electricity I underſtand that kind obſerved in animals, from whatever cauſe it may ariſe. When this electricity is excited in animals, neither by the peculiar movement of their own bodies, nor by friction, or the application of any other body, it is called Spontaneous Animal Electricity. We are taught by many inſtances, both ancient and modern, that men, as well as other animals, have exhibited evident ſigns of electricity; although the ancients, who mention theſe inſtances, did not know to what the phenomenon was to be aſcribed. It may not be improper, therefore, to quote here ſome of the moſt remarkable of them.

I. We are told by Virgil, that the hair of Aſcanius emitted a harmleſs kind of flame †.

> Ecce levis ſummo de vertice viſus Iuli
> Fundere lumen apex, tactuque innoxia molli
> Lambere flamma comas, et circum tempora paſci.
> Nos pavidi trepidare metu, crinemque flagrantem
> Excutere, et ſanctos reſtinguere fontibus ignes.

* From the *Tranſactions of the Electoral Academy of Sciences at Manheim*, Vol. VI.

† Æneid, Lib. II. v. 682.

II. We are informed by Dionyfius and other writers, that fire ftreamed forth from the hair of Servius Tullius, the Roman king, during fleep, when he was about feven years of age *.

III. Pliny alfo fpeaks of light often fhining around the heads of men:—*Hominum quoque capita vefpertinis horis ftellæ magno præfagio circumfulgent* †.

IV. We are told of a Carmelite monk, who was always attended with this phenomenon, that, as oft as he ftroked his hair backwards, it emitted fparks ‡.

V. A woman at Caumont exhibited a phenomenon of the like kind, as her hair, when combed in the dark, always emitted fire §.

VI. Father J. Faber fpeaks of a young woman from whofe head fparks of fire always fell when fhe combed her hair ||.

VII. Francifcus Guidus produced bright flames from his body when he rubbed his arm with his hand as he lay in bed ¶.

VIII. Ezekiel de Caftro **, a phyfician of Verona, relates the following circumftance refpecting Caffandra Buri, a lady of the fame place:—As often as fhe touched her body, even in a flight manner, with a linen cloth, it emitted fparks in great abundance, which could be perceived by every perfon ftanding near her, and which were attended with a confiderable noife. Her maids were often deceived by this phenomenon, and believed that they had, through careleffnefs, dropped fome coals between the fheets, as fhe always caufed her bed to be warmed in winter, at which time the fparks were moft abundant and ftrongeft.

IX. Anthony Cianfi, a bookfeller at Pifa, when he pulled off a narrow fhirt, and a piece of cloth which he wore on his breaft, emitted fparks from his back and arms, with a crack-

* Antiq. Rom. Lib. IV.
† Hift. Nat. Lib. II. cap. xxxvii.
‡ Cardanus, Lib. VIII. de rerum variet. cap. xliii.
§ Scaliger. Exer. 174.
|| In his Palladium Chymicum.
¶ Bartholinus de luce animalium, *Lugd. Bat.* 1647, p. 121.
** In his book *De igne lambente.*

ling

ling noiſe, to the great terror of his whole family. Fortunius Licetus was a witneſs of this phenomenon *.

X. "Among us," ſays Geſner, "where heated chambers are uſual, it often happens that many perſons, when they have warmed themſelves at a ſtove, and then pull off their ſhirt in a cold bed-chamber, or move or ſhake it after it is pulled off, obſerve crackling flames to burſt from it †."

XI. Bartholinus ſays, that a rope-dancer at Turin, according to the teſtimony of a reſpectable man, Caſſiano a Puteo, emitted a like phenomenon of light from his body ‡.

XII. The ſame author ſays, that ſparks proceed from the ſkins of cats, and particularly from the back; which can be clearly perceived by ſtroking the hair backwards even with the leaſt preſſure, and eſpecially after they have been warmed at the fire §.

XIII. Scaliger ſpeaks of a white Calabrian horſe, which, when combed in the dark, emitted ſparks of fire ||.

XIV. Ezekiel de Caſtro ſays ¶, of another horſe of the ſame kind, that real ſparks were obſerved when his neck was ſtroked upwards with the hand or a currycomb.

XV. Simpſon treats of the light emitted by the bodies of animals when rubbed; and quotes inſtances of ſuch phenomena on combing the hair of a woman, currying a horſe, and ſtroking a cat with the hand **.

XVI. The following circumſtance reſpecting himſelf was told by Vaudania to the celebrated Beccaria:—"For ten or twelve days paſt, ſince the cold ſet in, I wear, between two ſhirts, a piece of beaver's ſkin. Always when I pull off my upper ſhirt at night, I obſerve that it adheres, in ſome degree, to the piece of ſkin; and when I draw my ſhirt from it, I ſee ſparks which have a ſtriking reſemblance to thoſe of electricity. Scarcely do I begin to pull off the piece of ſkin

* Licetus de cauſſis monſtrorum, Lib. II. cap. xxviii.

† Lib. de lunariis.

‡ De luce animali, p. 123.

§ Ibid. p. 189.

|| Exercitat. 174.

¶ Liber de igne lamb.

** Diſſ. Phyſ. de fermentatione, 1675.

when I find that it adheres, and with ſtill greater force, to the under-ſhirt. On taking it out I obſerve, when I hold it in the right hand, that the frill of my ſhirt moves up from my body towards it. If I remove the piece of ſkin to a greater diſtance, and draw it from the frill, the latter moves again towards my body. If I bring the piece of ſkin nearer, the ſhirt moves again towards it. This oſcillation of my ſhirt between my body and the piece of ſkin continues alternately, till it is gradually leſſened, and at length ceaſes."

XVII. About twelve years ago the following circumſtance was communicated to our Academy, in writing, from Berdigheim :—On the 12th of February a young woman having put on an aired ſhift, which was exceedingly narrow and ſat very cloſe to her body, heard, on laying hold of it, a crackling noiſe like what oftimes proceeds from the flame of a candle, and obſerved ſparks to iſſue from all thoſe places which ſhe touched with her hands. Being aſtoniſhed at this phenomenon, ſhe called to another girl, who ſlept in the next apartment, to come to her aſſiſtance, as her ſhift, which ſhe in the mean time pulled off, was on fire. They both now ſhook the ſhift in order to extinguiſh the ſparks; but the more they moved it, the effects were ſtronger: on examining the ſhift, however, no traces of fire could be diſcovered. The firſt maid then put on another ſhift, which ſhe took from her box; but as ſoon as ſhe touched it, the ſame crackling noiſe and appearance of ſparks took place. She then put on her dirty ſmock, which ſhe had before pulled off, and returned to bed, on which the phenomenon diſappeared. Some perſons to whom the circumſtance was afterwards related, were deſirous to know whether the ſame phenomena would again appear if the maid put on another clean ſhift. She did ſo the following night; and as often as the ſmock was touched by her or any other perſon, a crackling noiſe was heard, and ſparks were emitted every time the finger was brought near it. The ſpectators now deſired her to put on another clean ſmock, which belonged to another maid: on approaching the hand a like noiſe was heard, and the appearance of fire was obſerved; but no ſparks iſſued from her body, even though touched, when ſhe had the ſhift on.

An

An experiment was then made to try whether the ſame phenomena would occur if another woman put on the girl's ſhift; but nothing of the kind was obſerved either by the perſon who had put it on, or by thoſe who touched it; but the phenomena were repeated when it touched the maid on whom they had been firſt obſerved. Every time after this period that the ſame perſon put on a clean ſmock, the ſame effects were produced; but they diſappeared after the ſhift had been worn ſome days. On the 1ſt of February the whole phenomenon ceaſed, and after that time never returned.

XVIII. M. Flad junior, member of our Society, having been in a conſumptive ſtate about eight years ago, obſerved, for a long time, as often as he pulled off or drew on his ſtockings, that ſparks proceeded from his feet in abundance. Having afterwards recovered his ſtrength in ſome meaſure, the ſparks diſappeared; but they returned when his ſtrength again began to decreaſe, and continued till his diſorder put an end to his exiſtence.

XIX. As often as M. Hertel, chaplain to the Elector, draws his hand over his breaſt, which is covered with hair, abundance of ſparks are ſeen in the dark.

XX. I was told by Count von Kagenek, that it often happened to him, that when he drew a ſilk handkerchief between his fingers, while ſtanding near a ſtove, long luminous ſtripes were here and there obſerved.

XXI. I was told alſo by a lady named Von Fraiſe, that very often, when ſhe rubbed her hands or arms even weakly with a linen cloth, lying in bed, abundance of ſparks iſſued from them.

XXII. M. Von Schlemmer, of Deux-Ponts, relates of a lady named Von Koch, that ſhe often obſerves, when ſhe gets up early, that a ribbon, with which ſhe binds up her hair during the night, adheres to her fingers when ſhe pulls it off, and moves with velocity towards other neighbouring bodies. Having, by my deſire, applied to the ribbon a piece of ſealing wax, which had been rubbed, it was always repelled by it.

XXIII. I am acquainted with another lady, whoſe hair, when

when combed in the dark, always has a luminous appearance, as I have ſometimes obſerved myſelf.

XXIV. I have a white horſe, from the body of which, particularly in the winter-time, the comb brings forth abundance of ſparks. I have likewiſe a white dog, from the back of which, when I draw my hand over it, in a warm room, from the tail to the head, ſparks iſſue with a ſnapping noiſe, and which ſeem ſo troubleſome to the animal, that he endeavours to run away. I have ſometimes charged a jar with them.

XXV. I have often produced the ſame phenomenon on an aſh-grey, ſtrong, lively cat.

XXVI. We are acquainted with three kinds of fiſh which, when touched, give a ſhock almoſt like that occaſioned by a charged jar, *viz.* the cramp-ray, *Raia torpedo* L.; the electrical eel, *Gymnotus electricus* L.; and the *Silurus electricus* L. The more modern philoſophers have convinced themſelves, by repeated experiments, that the effects of the ſhock given by theſe animals are of an electric nature, though to me it appears probable that they depend, in part, on another cauſe.

The animal electricity which I have announced in theſe numerous inſtances is of the coarſer kind, and may be eaſily obſerved by every one; but there is another kind, of a more delicate nature, which does not ſpontaneouſly manifeſt itſelf, but muſt be concentrated and called forth by art. This finer electricity I find firſt mentioned in a letter of M. de Sauſſure to the editor of the *Journal de Phyſique* at Paris. The experiments which this celebrated philoſopher made, partly on himſelf and partly on others, by means of Volta's electrometer and condenſer, are given there only in extracts; but the conſequences he deduces from them are as follows:

In order to produce this electricity, bodily motion is neceſſary. The electricity thence produced ariſes from the friction of the body againſt the clothes; for as often as he made the experiment naked, he found no electricity. In order that electricity may be produced by the friction of the body againſt the clothes, the latter muſt poſſeſs the natural warmth of the body; for when he had on clothes that were cold, he could never perceive the leaſt trace of electricity: no elec-

tricity

tricity appears also when the body is in a state of perspiration. There are persons who, in this manner, never emit electricity. The electricity which shews itself in the human body is positive, and sometimes negative. The cause of this variation he was not able to discover.

[To be concluded in next Number.]

II. *On the Method of Distilling as practised by the Natives at Chatra in Ramgur, and other Parts of India.* By ARCHIBALD KEIR, *Esq.**

THE body of the still they use is a common, large, unglazed, earthen water-jar, nearly globular, of about twenty-five inches diameter at the widest part of it, and twenty-two inches deep to the neck, which neck rises two inches more, and is eleven inches wide in the opening. Such, at least, was the size of the one I measured; which they filled about a half with fomented Mâhwah flowers, that swam in the liquor to be distilled.

The jar they placed in a furnace, not the most artificial, though seemingly not ill adapted to give a great heat with but very little fuel. This they made by digging a round hole in the ground, about twenty inches wide, and full three feet deep; cutting an opening in the front, sloping down to the bottom, on the sides perpendicular, of about nine inches wide and fifteen long, reckoning from the circle where the jar was to come, to serve to throw in the wood at, and for a passage to the air. On the side too they cut another small opening of about four inches by three; the jar, when placed, forming one side of it, to serve as a chimney for the smoke to go out at. The bottom of the earth was rounded up like a cup. Having then placed the jar in this, as far as it would go down, they covered it above, all round, with clay, except at the two openings, till within about a fifth of its height; when their furnace was completed.

In this way I reckon there was a full third of the surface

* From the *Asiatic Researches.*

of

of the body of the ſtill, or jar, expoſed to the flame, when the fire came to be lighted; and its bottom, not reaching to within two feet of where the fuel was, left a capacious hollow between them, whence the wood, that was ſhort and dry, when lighted, being moſtly converted into flame, and circulating on ſo great a ſurface of the ſtill, gave a much ſtronger heat than could elſe have been produced from ſo very little fuel: a conſideration well worth the attention of a manufacturer, in our country more eſpecially, where firing is ſo dear. There indeed, and particularly as coal is uſed, it would be better, no doubt, to have a grate; and that the air ſhould enter from below. As to the benefit reſulting from the body of the ſtill being of earthen-ware, I am not quite ſo clear in it. Yet, as lighter ſubſtances are well known to tranſmit heat more gradually and ſlowly than the more ſolid, ſuch as metals, may not earthen veſſels, on this account, be leſs apt to burn their contents, ſo as to communicate an empyreumatic taſte and ſmell to the liquor that is diſtilled, ſo often, and ſo juſtly complained of with us? At any rate, in this country, where pots are made ſo cheap, I ſhould think them greatly preferable, as at leaſt much leſs expenſive than thoſe which the gentlemen engaged in this manufacture moſt commonly employ: though of this they are beſt able to judge.

Having thus made their furnace, and placed the body of the ſtill in it, as above deſcribed, they then luted on, with moiſtened clay, to its neck, at the opening, what they here call an *adkur*; forming with it, at once, a cover for the body of the ſtill, with a ſuitable perforation in it to let the vapour riſe through, and the under-part of the alembick. The *adkur* was made with two earthen pans, having round holes in their middles of about four inches diameter; and, their bottoms being turned oppoſite the one to the other, they were cemented together with clay; forming a neck of junction thus of about three inches, with the ſmall riſing on the upper pan. The lowermoſt of theſe was more ſhallow, and about eleven inches wide, ſo as to cover exactly the opening at the neck of the jar, to which they luted it on with clay. The upper and oppoſite of theſe was about four inches deep,

and fourteen inches wide, with a ledge round its perforation in the middle, riſing, as is already ſaid, from the inner ſide of the neck, of about half an inch high, by which a gutter was formed to collect the condenſed ſpirit as it fell down; and from this there was a hole in the pan to let it run off by; to which hole they occaſionally luted on a ſmall hollow bamboo, of about two feet and a half in length, to convey it to the receiver below. The upper pan had alſo another hole in it, of about an inch ſquare, at near a quarter of its circumference from the one below juſt ſpoken of, that ſerved to let off the water employed in cooling; as ſhall be mentioned preſently.

Their *adkur* being thus fitted to the jar, they completed the alembic by taking a copper pot, ſuch as we uſe in our kitchens, of about five inches deep, eight wide at the mouth, and ten at the bottom, which was rather flattiſh; and turning its mouth downward, over the opening in the *adkur*, luted it down on the inſide of the jar with clay.

For their cooler they raiſed a ſeat, cloſe upon, and at the back part of the furnace, about a foot higher than the bottom of the copper pot. On this they placed a two or three gallon pot, with a round hole, of about half an inch, in the ſide of it; and to this hole, before they lighted their fire, they luted on a ſhort tube of a like bore; placing the pot, and directing its ſpout, ſo as that, when filled with water, it threw a conſtant and uniform ſtream of it from about a foot high, or near the centre of the bottom of the copper pot, where it was diffuſed, pretty completely, over its whole ſurface; and the water falling down into the upper part of the pan of the *adkur*, it thence was conveyed, through the ſquare hole already mentioned, by a trough luted on to it for that purpoſe, to a cooling receiver a few feet from the furnace; from which they took it up again, to ſupply the upper pot as occaſion required.

As their ſtock of water, however, in this ſort of circulation, was much ſmaller than it ſeemingly ought to have been, being ſcarcely more than ſix or eight gallons, it too ſoon became hot; yet, in ſpite of this diſadvantage, that ſo eaſily might have been remedied, and the ſhortneſs of the conducting tube, which had nothing but the common air to

cool it, there ran a ſtream of liquor from the ſtill, and but very little vapour riſing from it, beyond any thing I had ever ſeen from ſtills of a much larger ſize, fitted with a worm and cooler. In about three hours time, indeed, from their lighting of the fire, they drew off full fifteen bottles of ſpirit; which is more by a great deal, I believe, than could have been done in our way from a ſtill of twice the dimenſions.

The conveniences of a worm and cooler, which are no ſmall expence either, I have myſelf often experienced; and if theſe could be avoided in ſo ſimple a way that might eaſily be improved, the hints that are here offered may be of ſome uſe. The thin metal head is certainly well adapted, I think, to tranſmit the heat to the water, which is conſtantly renewed; and which, if cold, as it ought to be, muſt abſorb the faſteſt poſſible: whereas, in our way, the water being confined in a tub, that, from the nature of its porous ſubſtance, in a great degree, rather retains than lets the heats paſs away, it ſoon accumulates in it, and becomes very hot; and, though renewed pretty often, never anſwers the purpoſe of cooling the vapour in the worm ſo expeditiouſly and effectually as is done by their more ſimple and leſs expenſive apparatus. In this country, more eſpecially, where labour and earthen-wares are ſo cheap, for as many rupees, and leſs, twenty furnaces, with ſtills, and every thing belonging to them, independent of the copper pots, might very well be erected, that would yield above a hundred gallons of ſpirits a-day; allowing each ſtill to be worked only twice. So very cheap indeed is arrack here, to the great comfort of my miners, and of many thoughtleſs people beſide, that for one ſingle *peyſa* (not two farthings ſterling) they can get a whole *cutcha-ſeer* of it in the *baſar*, or above a full Engliſh pint, and enough to make them completely intoxicated; objects often painful to be ſeen.

Of the ſuperior excellence of metal in giving out heat from itſelf, and from vapour contained in it, we have a very clear proof in what is daily performed on the cylinder of the ſteam-engine: for, cold water being thrown on it when loaded, the contained vapour is conſtantly condenſed; whence, on a vacuum being thus formed, and the weight of the atmo-

ſphere

ſphere acting on the ſurface of the piſton attached to the arm of the balance, it is made to deſcend, and to raiſe the other arm that is fixed to the pump; while this, being ſomewhat heavier, immediately ſinks again, which carries up the piſton, while the cylinder is again filled; and thus, by alternately cooling and filling it, is the machine kept in motion; the power exerted in raiſing the pump-arm being always in proportion to the diameter of the cylinder, or to the ſurface of the piſton, which is exactly fitted to it, and on which the preſſure acts.

The contrivance, too, of having the under part of the alembic, where the condenſed vapour is collected, or upper part of what they call the *adkur*, of earthen-ware, of ſo great a thickneſs, and of courſe at ſo great a diſtance from the heat in the body of the ſtill, is well imagined to keep the ſpirits the cooleſt poſſible, when collected, and running off.

By thus cooling and condenſing the vapour, likewiſe, ſo ſuddenly as it riſes, there is, in a great meaſure, a conſtant vacuum made, or as much as poſſible can be; but, that both ſteam riſes faſter, and that water boils with much leſs heat when the preſſure is taken away from its ſurface, is an axiom in chemiſtry too well known to need any illuſtration; it boiling in vacuum when the heat is only ninety or ninety-five by Farenheit's thermometer; whereas in the open air, under the preſſure of the atmoſphere, it requires no leſs than that of two hundred and twelve, ere it can be brought to the boiling point.

I muſt further obſerve, that the ſuperior excellence of condenſing the vapour ſo effectually and ſpeedily in the alembic to our method of doing it in a worm and cooler, is greatly on the ſide of the former; both from the reaſons I have already adduced, and becauſe of the ſmall ſtream of vapour that can be only forced into the worm, where it is condenſed gradually as it deſcends; but, above all, from the nature of vapour itſelf, with reſpect to the heat contained in it, which of late has been proved, by the very ingenious Doctor Black, to be greater by far than, before his diſcoveries, was imagined. For vapour he has ſhewn to be in the ſtate of a new fluid, where water is diſſolved by heat; with the aſſiſtance,

perhaps, if I may be allowed a conjecture, of the air which it contains; and all fluids, as he has clearly demonstrated, on their becoming such, absorb a certain quantity of heat, which becomes what he very properly calls latent heat; it being heat not appearing either to the senses or to the thermometer while they remain in that liquid state, but showing itself immediately by its effects on whatever is near it; upon their changing their form from fluid to solid, as on water becoming ice, or metals fixing, and the like. In the solution of salts, also, there is an absorption of heat, as we daily experience in the cooling of our liquors by dissolving saltpetre in water; and this he has found to be the case with water itself, and other fluids, when passing into a state of vapour by boiling. From the most accurate and judicious experiments, indeed, he infers, and with the greatest appearance of truth, that the heat thus concealed in vapour raised by boiling, from any given bulk of water, would be fully sufficient, if collected in a piece of iron of the like size, to make it perfectly red hot. What then must be the effect of so much heat communicated in our way of distilling to the worm, and to the water in the tub, will be sufficiently evident, from what has been said, to prove, I think, that we have hitherto employed a worse and more defective method than we might have done with respect to cooling at least, both in the making of spirits, and in other distillations of the like kind, where a similar mode is adopted.

The poor ignorant Indian, indeed, while he with wonder surveys the vast apparatus of European distillers, in their immense large stills, worms, tubs, and expensive furnaces, and finds that spirits thus made by them are more valued, and sell much dearer than his own, may very naturally conclude, and will have his competitors join with him in opinion, that this must alone surely be owing to their better and more judicious manner of distilling with all those ingenious and expensive contrivances, which he can no ways emulate: but in this, it would appear, they are both equally mistaken; imputing the effects, which need not be controtroverted, perhaps, to a cause from which they by no means proceed; the superiority of their spirits not at all arising from the

the ſuperior excellence of theſe ſtills and furnaces, nor from their better mode of conducting the diſtillation in any reſpect; but chiefly rather from their greater ſkill and care in the right choice and proper management of the materials they employ in fermentation; and, above all, as I apprehend, from the vaſt convenience they have in caſks, by which, and from their abilities in point of ſtock, they are enabled, and do in fact, in general, keep their ſpirits for a certain time, whence they are mellowed, and improved ſurpriſingly both in taſte and ſalubrity.

All I need further add with reſpect to diſtillation, and on the ſuperior advantages in the mode of conducting it here, to that we have been in uſe to employ, for the raiſing of ſpirits, ſimple waters, and the like, is only to obſerve, I have no ſort of doubt but that the intelligent chemical operators at home, if ever they ſhould get a hint of it, will make no manner of ſcruple to uſe it alſo, and to improve upon it greatly by a few ingenious contrivances, which their knowledge and experience will ſo eaſily ſuggeſt. The principles on which it ſeems founded, indeed, eſpecially with regard to their way of cooling, are ſo ſtriking and juſt, that in many other diſtillations beſides thoſe of ſpirits and waters, they may be employed, I apprehend, with very great profit and advantage. I ſhall now, however, confine myſelf to mention only the benefit that may reſult from a like proceſs in the raiſing of the finer aromatics, while the heat contrived, as in our way, beſides impeding the diſtillation, muſt, from its long action on ſuch ſubtile bodies, probably injure them greatly in the eſſential quality on which their excellence depends; and upon this very account I am apt to imagine that the greater quantity obtained, and the ſuperior quality of the oil of roſes made in this country, to that made from roſes with us, is owing chiefly, if not entirely, to their better and more judicious manner of extracting it here. For with us, the ſtill, being made of metal, may, in the firſt inſtance, impart too great and too ſudden a degree of heat; and next, the oil continuing ſo long in the vapour, and that much compreſſed, may, in ſo delicate a ſubject, not only entirely almoſt unite it with the water, ſo as to render the ſeparation

tion impracticable, but may at the same time alter its essence so completely, as that it can no longer appear in the state it otherwise might have been found in, had the operation been better conducted, or in the way they do here. A very few trials, however, would much better certify this than all I can possibly say on the subject, or, in fact, than all the reasoning in the world. Therefore, as to my own particular opinion of the flavour and quality of the roses at home being equal, if not superior, to that of those in this country, I may be entirely silent; the rules and reasoning in chemistry, though serving greatly to enlarge and improve our understanding, being what of themselves can never be depended upon till confirmed by facts and experiments; where many things often turn out very different from what, from our best and most plausible arguments, we had the greatest reason to expect. Or, if it should be found to be really true, what I have often heard asserted, by those however who had it only from others, but not of their own particular knowledge, that, in distilling their oil of roses at the places where they make it the best, they use also with their roses, sandal wood, and some other aromatics, no roses whatsoever, it is plain, could ever of themselves be made to afford a like oil, nor without such an addition as they employ. A circumstance, by the bye, that might possibly easily be certified by some one of the many ingenious correspondents of the Society who may happen to reside where it is made; and a knowledge of the real truth of it would certainly be of use.

III. *On the Method employed at Fez and Tetuan to prepare those Goat Skins called Morocco. By* C. A. BROUSSONET*.

THE skins, after being flayed from the animals by stripping them off entire, are immersed in water for three days: they are then exposed to the air, and, when dry, the hair is taken off, but imperfectly. They are then dipped in slacked lime, and sprinkled over with powdered lime to detach the

* From the *Bulletin des Sciences*, No. 23.

ſmaller hairs; after which they are waſhed in running water, and rinſed with care. After remaining a night in the water, they are dried in the open air. Thirty parts of theſe ſkins are then placed in two quintals of bran, (each part conſiſts of ſix ſkins, and the quintal is 150 pounds.) They are ſuffered to continue in this ſtate, turning them every day, till they have acquired a great degree of pliability. They are afterwards waſhed again in running water, and trod upon with the feet; then thrown into a ſecond bath made with white figs, about a quintal and a quarter of which are employed for thirty parts of ſkins. The figs render the water ſaponaceous. They are left in this bath four or five days, during which they are often turned; and, while immerſed in the water, they are for three days beſprinkled with finely pounded rock-ſalt. The water is then drained off, and, being again beſprinkled with ſalt, they are placed in a heap in a flat veſſel, where they imbibe the ſalt completely. The water they contain is wrung out by twiſting them; and they then become exceedingly pliable, and fit for receiving the colour.

If deſtined to be red, half a pound of cochineal and three ounces of alum are employed for ten parts of ſkins. They are then put into pits, where tan-beds are formed of about fifty pounds for each ſkin; which is turned in ſuch a manner that the grain is inſide, and that the ſkin may be filled with the tanning water. At the end of eight days they are turned inſide out, and are ſtill filled with the tanning water, in which they are left for ſix days; care being taken to ſtir them. They are then rinſed in running water; ſcraped with an iron inſtrument; cut up along the belly; and ſoftened with a little oil. They are dried in the ſun, and then cooled in the ſhade: after which they are gently beſprinkled with water, and the edges are paired off with three different inſtruments of iron. When the red is too dark, a decoction of a plant called *razoul al acbbi*, a kind of annual *meſembrianthemum*, is employed to weaken it. This liquid is applied warm, by pouring a ſpoonful over each ſkin.

If the morocco is intended to be yellow, the ſkins are prepared as for the red, but not ſalted till they are put into the fig-bath; and for five dozen of ſkins, twenty-five pounds of

tan

tan only are employed. They are dyed with the pulverised bark of the pomegranate tree and alum.

The false red dye is communicated to skins with Brazil wood and alum. Instead of Brazil wood, *fouah*, a kind of *galium* or *rubia* (madder), brought in large quantities from Morocco, is often employed.

IV. *Recipe for destroying Caterpillars on Gooseberry Bushes*.*

A RECEIPT for this purpose was offered to be communicated to the Society by William Henderson, of Baldridge Burn near Dunfermline, on the 6th of February 1795, for a suitable reward. The proposal was referred to a Sub-Committee, of which Dr. Monro, Professor of Anatomy in the University of Edinburgh, was chairman, who, after making trial of the receipt, gave in their report on the 1st of July 1796. The receipt for the preparation, and the manner of using it, was in the following words:—

Take one Scots pint of tobacco liquor †, which the manufacturers of tobacco generally sell for destroying bugs, and mix therewith about one ounce of alum; and when the alum is sufficiently dissolved, put this mixture into a plate, or other vessel, wide and long enough to admit of a brush, like a weaver's brush, being dipped into it; and as early in the season as you can perceive the leaves of the bushes to be in the least eaten, or the eggs upon the leaves, (which generally happens about the end of May,) and which will be found in great numbers on the veins of the leaves on their under side; you are then to take the preparation, or liquor, and after dipping the brush into it, and holding the brush

* From *Prize Essays and Transactions of the Highland Society of Scotland*, Vol. I.

† Tobacco liquor is the superfluous moisture expressed from roll tobacco in the operation of pressing it; and is, in fact, only a strong infusion of tobacco in well or spring water, which may be made, where it cannot be purchased, by infusing any kind of tobacco in water till all the strength be extracted. Perhaps the sulphat of iron (copperas), employed in dyeing the roll tobacco, contributes a little to the efficacy of the liquor: a little of it may therefore be added to the infusion. Edit.

towards

towards the under side of the bush, which is to be raised and supported by the hands of another person; and by drawing your hand gently over the hairs of the brush, the above liquid is sprinkled and falls in small drops on the leaves: the consequence of which is, if the eggs are there, they never come forward; and if they have already generated worms, in a minute or two after the liquor touches them, they either die, or sicken so as to fall off the bush, at least they do so upon giving it a little shake. If, upon their thus falling off, they shall not appear to be completely dead, the bush should be held up, and either a little boiling water from a watering-pan thrown on them, or a bruise given them by a spade or shovel, or the earth where they lie turned over with a hoe. This preparation does not in the least injure the bushes.

The liquor here meant is generally not in the same state it is extracted from the tobacco, but is mixed by the tobacco manufacturers with cold water, in the proportion of four or five pints of water to one of the original juice or essence. Therefore, any person who may purchase the juice itself, unmixed, must mix it with water in the above proportion, and the quantity of alum must be about an ounce for each Scots pint of the mixture.

Dr. Monro's report was in the following words:—I observed, along with Mr. Hamilton and Mr. Gordon, (two other gentlemen of the committee,) and two gardeners who were present, that such caterpillars as were wetted by the liquor Mr. Henderson employs, were killed in a very few minutes, and the experiment has been repeated by my own gardener with the same effect. I have likewise found, that it kills a kind of green fly, which is very hurtful to the leaves of plum-trees and other fruit-trees. It has been very generally known, that the smoke and the juice of tobacco were pernicious to different kinds of insects and worms; but it has not, so far as I know, been employed in Mr. Henderson's manner; and as this has the advantage of not hurting the leaves, nor the fruit, I consider it as an useful and material improvement, well entitled to a moderate premium. (Signed) ALEX. MONRO, M.D.

V. *On the gradual Changes in Temperature and Soil which take place in different Climates, with an Enquiry into the Cause of those Changes.* By the *Abbé* MANN.

[Concluded from Page 347 of the last Volume.]

II. *Physical Causes of the gradual Alterations in Soil and the Temperature of the Climate.*

I HAVE no doubt that a great number of different causes may have contributed, each its part, more or less, to produce those effects which are the object of the present research. Some of them are only accidental, and have taken place in different countries at very different periods, while in others some of them have not taken place at all. Among these are the draining of lakes and morasses, the extirpating of forests, and the cultivation of land. All these circumstances, no doubt, render the temperature of climate milder. I found, however, one cause, of a kind altogether different, which appears to me general and uniform in producing the above changes. After what I have before said, and in another place*, it may be readily conjectured that I here speak of an union of the two distinct principles, moisture and heat. Their mutual disengagement, and the increase of the one above the other, afford, if I am not mistaken, a key to a true theory of the earth.

All the ancient writers who speak of the countries of Europe beyond the latitude of 50° north, represent them as filled with lakes and morasses, and covered with immense forests, almost as America is at present. It is a certain fact, that the climate of North America is different from that of Europe by about ten degrees of longitude; that is, the districts of North America lying under 40° of north latitude are as cold and moist as the countries of Europe which lie in the latitude of 50°. New England lies between the 41st and 46th degrees of latitude; yet it is observed that the climate there, in regard to heat and cold, is equal to that of

* See my Memoir on the Ancient State of Flanders in *Memoires de l'Acad. de Bruxelles*, Vol. I. p. 67—72.

the

the districts of England between the latitude of 50 and 56. It is well known that the people of America are more and more extirpating the forests, draining the marshes, and cultivating the land; and that the climate there is found to become perceptibly milder. For a thousand or two thousand years past the people in all the northern parts of Europe have been in the same manner employed in the improvement of the soil. These causes, however accidental they may be, and however much dependant on human industry, must certainly have contributed their part to render the climate milder, not only in the countries where they took place, but even in the neighbouring countries, exposed to the effects of their atmosphere.

It is almost needless to observe, that the great number of lakes and morasses, which, according to the accounts of ancient authors, existed in their time in the southern parts of Europe, must have rendered the air of these countries exceedingly cold and moist as well as unhealthful, since it lessened its elasticity, and filled it with thick vapours; which corresponds with the description they have given us of them. The countries of Europe have not for a long time been seen covered in that manner with lakes and morasses, if we except Sweden and Norway; though the places where such marshes formerly existed, both in England and on the Continent, in Gaul, Germany, and the European Sarmatia, may be still clearly observed. It is certain that human industry, in the course of ages, has in part contributed to produce this change, as a great many epochs are known when the draining of lakes and morasses was undertaken; but I have no doubt that gradual sinking of the surface of the sea, which occasions a natural and gradual efflux, that could be produced only by these means, may have contributed its part also. But, from whatever cause these changes may have proceeded, it is certain that they have contributed to lessen the moisture and cold of all the countries of Europe.

We know that in the time of Julius Cæsar, and even long after, almost all Germany and Sarmatia were covered with immense forests. The Hercynean forest was sixty days journey in length. It began in Belgic Gaul near the se[illegible],

and extended through Germany and Poland. England was proportionally lefs abundant in forefts. Now it may be readily comprehended what extraordinary cold, what moift and unhealthful air muft have prevailed in the climate of thefe extenfive countries, as all the mountains and plains were covered with fuch immenfe woods, and as each valley almoft contained a lake or a marfh; and what wonderful changes in the temperature of thefe lands muft have been effected by the extirpation of thefe extenfive forefts, and by draining off the ftagnant waters. Large woods prevent the beams of the fun from penetrating to, and warming the foil; they impede alfo the free diffufion of the internal heat, as the fallen leaves and branches which rot on the ground form a moift cruft through which the internal and external heat can with difficulty force a paffage. In the laft place, they concentrate the cold and moift vapours, render them putrid, and corrupt the whole atmofphere. This has been always obferved in North America, as we are affured by Dr. Williamfon; and the confequences are bilious and intermittent fevers in fummer and autumn, and inflammatory fevers in winter. He afferts, that the opener and drier the land becomes, the more it is remarked that thefe fatal difeafes decreafe. This muft have been the cafe formerly in Europe under the like circumftances, and the like caufes muft have contributed to render its climate milder and more falubrious.

The Celts and Sarmatians, who were the firft inhabitants of all the European countries lying to the north of Italy and Greece, like all the barbarous nations under different names which defcended from them, and which over-ran the Roman empire in the fifth and fixth centuries, defpifed agriculture, and cultivated no more land than was fufficient to fupply the wants of the current year. They lived chiefly either on what they caught in hunting, or the flefh of their domeftic animals, of which they reared a great many; as they confidered, though very unjuftly, thefe employments as much nobler than the cultivation of land. Now it is certain that the culture of the earth, which breaks its furface, puts it in movement, keeps it in a ftate of continual tendernefs, and makes it capable of imbibing the rays of the fun in fummer, and of affording

affording a paſſage to the internal heat in winter; and by theſe means contributes to preſerve a continual equilibrium of the principle of heat in the earth and the atmoſphere. The contrary takes place in all uncultivated countries, eſpecially when they are moiſt, and covered with wood.

It can no longer be doubted that the gradual draining of the ſtagnant water in all Celto-Scythia and European Sarmatia, with the extirpation of their large foreſts, and the general cultivation of the fields of theſe countries, muſt have had an influence alſo on the atmoſphere of Greece and Italy. Thoſe cutting north-winds which converted every thing into ice, and of which the Greeks and the Romans complain ſo much, have, in a great meaſure, ceaſed ſince the principal cauſes which produced them no longer exiſt. As long as Germany, Pannonia, Dacia, Mœſia and Thrace remained uncultivated and covered with immenſe foreſts, their atmoſphere was exceedingly cold, thick and heavy, and had a continual influence on that of Italy and Greece, in which, becauſe they were open and warm countries, the atmoſphere was conſequently far lighter. The exertions of this fluid to recover its equilibrium were the cauſe of the cutting north winds, of which the Greeks and the Romans complained ſo much. But after thoſe centuries, when the whole of Celto-Scythia and Sarmatia became opener and better cultivated, their atmoſpheres muſt have come nearer to an equilibrium with that of Greece and Italy, and conſequently theſe ſtreams of air from the north muſt have decreaſed in the ſame proportion. This muſt have contributed to moderate the climate of Greece and of Italy, and to render it much milder than it was about 1800 or 2000 years ago; and to ſuch a degree, that, had there been no other cauſe, we could no longer wonder at, or entertain any doubt of, the effects of the cold which the ancients remarked in their time, and which are not obſerved at preſent.

It is well known that the winds which traverſe the immenſe cold regions of the Continent are always colder and more penetrating than thoſe which blow from the ſea. America extends a great way towards the north; and its remote diſtricts, on account of the ice, ſnow, and continual

fogs

fogs which prevail there, have never been explored, and therefore remain unknown. This immenſe country certainly extends nearer the north pole than the Continent of Europe and Aſia, the boundaries of which are pretty well known. This, without doubt, may be reckoned among the cauſes which render North America much colder than Europe under the ſame degrees of latitude, as I have already remarked. Hence it happens that the immenſe Continent of America, the farther it extends towards the north, is more filled with lakes and marſhes, which muſt conſequently add to the ſeverity of the winds that ſweep their ſurface.

It will perhaps be ſaid, that it appears to follow, from my poſitions, that the more the cold of winter decreaſes, from the above cauſes, the heat of ſummer ought to increaſe in the ſame proportion. In my opinion it can be proved by many monuments, hiſtorical as well as phyſical, that the ſum-total of the mean ſummer heat is greater than it was formerly, and that it continually increaſes; though this takes place imperceptibly, and can be obſerved only after periods of conſiderable length, and by comparing the reſpective degrees of diſtant epochs. On the other hand, in regard to the degree of the intenſity of that penetrating, concentrated, and ſtifling heat which is experienced even in Lapland, I muſt ſay, that this kind of heat is leſſened by all thoſe cauſes before enumerated which leſſen the cold of winter. I by no means aſcribe contrary effects to the ſame cauſe. We are taught by general experience, that the thinner, purer, and more elaſtic the air is, the leſs, in the ſame proportion, is the intenſity of the ſummer heat; and, on the contrary, the thicker the atmoſphere is, and the more it is filled with ſtagnant and concentrated vapours, the heat is more intenſe and the more ſtifling. For this reaſon it is always cooler on the ſummits of high mountains, while a ſtifling and inſupportable heat prevails in the neighbouring plains, eſpecially when they are ſurrounded by wood. This is always obſerved in the ſavannahs of America. Dr. Williamſon was convinced, by experiments and obſervation, of the truth of my aſſertions in regard to North America; and he remarks, that when this extenſive country becomes entirely open, when its

woods are cut down and its plains cultivated, the feverity of the winter cold will not only decreafe, but the ftifling unhealthful heat of the fummer will be moderated. The quantity of the fnow, ice, and moifture is already evidently leffened; and many plants, which could not be cultivated there formerly, now thrive and fucceed.

I now come to the laft and principal caufe of thefe changes, which acts in a general and uniform manner in producing thefe effects, as all the others are merely accidental, and depend on human induftry. I here mean to fay, that the principle of heat, increafed continually in the courfe of time, fo as to overcome the oppofite principles of moifture and cold, renders, by thefe means, the earth drier and fuller of ftones, and confequently increafes the fum of the degree of heat. Without this principle, in my opinion, we can never find fufficient grounds for the wonderful changes which have taken place in the nature of the foil of all thofe lands which border on the Mediterranean fea, which formed the ancient empire of Rome from Syria to India, and which at prefent have all become uncommonly fruitful, dry and ftony, as I have already remarked. The mere neglect of agriculture could never have produced thefe effects, and muft have been attended rather with effects of a contrary nature *.

VI. *Agenda*,

* The author here fubjoins the different paffages of ancient authors alluded to in the courfe of this paper; but as thefe would occupy too much room, we muft content ourfelves with giving only the references, which will no doubt be acceptable to our learned readers, who may be defirous of profecuting this fubject farther:—Herodotus (469 years before the birth of Chrift), Lib. IV. cap. 28, 29; M. Terentius Varro (72 years before J. C.), De re ruftica, Lib. I. cap. 7; C. Jul. Cæfar (52 years before J. C.), De bello Gallico, Lib. IV. cap. 1; Virgil (50 years before J. C.), Georgic. Lib. III. v. 349—383; Georgic. IV. v. 125, 135; Diodorus Siculus, (45 years before J. C.), Biblioth. Hift. Lib. V. cap. 25; Ovid (10 years before J. C.), Trift. Lib. III. Eleg. IV. v. 48, 49. 51; Eleg. X; Strabo (28 years after J. C.), Geograph. edit. *Bafil*, 1539, Lib. II. p. 67, 68. 107. 119, Lib. VII. p. 297; Pomponius Mela (40 years after J. C.), De fitu orbis, Lib. II. cap. 1. De Scythia Europæa; cap. 2. De Thracia; Lib. III. cap. III. De Germania; Columella (43 years after J. C.), De re ruftica, edit. Stephani 1543, in præf. n. 7, 8. Lib. I. cap. 1. p. 11, 12; L. Ann. Seneca (55 years after J. C.), De providentia, cap. 4. edit. *Lugd. Bat.* Tom. i. p. 711; Petronius Arbiter (60 years after J. C.), Satyr.

VI. *Agenda, or a Collection of Observations and Researches, the Results of which may serve as the Foundation for a Theory of the Earth.* By M. DE SAUSSURE.

[Continued from page 359 of the last Volume.]

CHAP. XXI.

Researches to be made in regard to the Loadstone.

1. THE theory of the loadstone ought to form a part of the theory of the earth, because the phenomena which depend on it belong to the whole globe; and because Halley, and after him other philosophers, have endeavoured to explain the different phenomena of the magnet by supposing the earth to be hollow, and that it contains in its cavity one or more magnetic globes.

2. In considering the loadstone it ought first to be examined whether, in order to explain its phenomena, we ought, like Descartes, to suppose a close fluid moving in a vortex around the magnet, and entering at one of its poles and issuing at the other; or, as M. Æpinus, a discrete fluid, susceptible of rarefaction and condensation, which is rarefied in one of the poles and condensed in the other; or, lastly,

Satyr. p. 10; Pliny the elder (74 years after J. C.), Hist Nat. edit. *Basil* 1525, fol. Lib. IV. cap. 12. p. 60; Papinius Statius (85 years after J. C.), Sylv. Lib. V. p. 83. edit. *Amst.* 1624; Tacitus (97 years after J. C.), De moribus Germanorum, cap. 2. 4. 5; Pliny the younger (99 years after J. C.), in Panegyr. cap. 12; Plutarch (101 years after J. C.), De fluviis, Tom. II. p. 1156. edit. *Frankf.* 1620, p. 949; L. A. Florus (102 years after J. C.), Lib. IV. cap. 12. edit. *Elzev.* 1610, p. 440; Appianus (130 years after J. C.), Excerpta ex ejus Celticis a Valesio, p. 1220; Pausanias (174 years after J. C.), Arcad. cap. xvii. p. 634. edit. *Leipf.* 1696, fol.; Dio Cassius (238 years after J. C.), Hist. Lib. XLIX. p. 413. edit. *Hanov.* 1606. fol.; Herodian (229 years after J. C.), Hist. edit. *Oxon.* Gr. & Lat. 1699.8. Lib. I. p. 12. Lib. VI. p. 221; Justin (250 years after J. C.), Histor. T. II. cap. ii. p. 25. edit. *Elz.* 1664; Ammianus Marcellinus (370 years after J. C.), Lib. XIX. cap. 11; Jorrandes (525 years after J. C.), De rebus Geticis, cap. liv. p. 693. edit. *Amst.* 1655; Xiphilinus (1083 years after J. C.), in Epit. Dionis Cassii, Lib. LXVIII. p. 776, and Lib. LXXI. p. 804.

as

as M. Prevoſt *, two fluids, ſuſceptible of being combined with each other in ſuch a manner that one of them alone is accumulated around the north pole of a magnet, while the other is accumulated around the ſouth pole; and that all the magnetic phenomena may be explained by the elective attractions which theſe fluids exerciſe either upon each other or on iron †.

3. It muſt then be examined, whether the direction of the magnetic needle, and its inclination, depend on the ſituation of a large magnet encloſed in the bowels of the earth, as Halley ſuppoſes; or on the action of one of theſe two magnetic fluids towards one of the poles, and, perhaps, of the other fluid towards the oppoſite pole, as M. Prevoſt ſuppoſes.

4. If we admit the hypotheſis of a large magnet ſuſpended within the cavity of the earth, ſhall we ſuppoſe, as the inventor of this hypotheſis, that this magnet has four poles? Or, ſhall we endeavour to explain the whole, as that great geometer Euler has done, by a magnet having only two poles? Or, laſtly, ſhall we ſuppoſe, as Mr. Churchman, an American philoſopher, has lately done, that the earth contains two magnetic poles, one at the north and the other at

* De l'origine des forces magnétiques, 8. *Geneve* 1788.

† The celebrated Coulomb admits alſo two fluids, which compoſe the magnetic fluid, and which exerciſe their action in the inverſe ratio of the ſquare of the diſtance: but, in the theory of phenomena connected with the action of the globe, this philoſopher ſets out from certain facts immediately given by obſervation; one of which it would be of importance to verify in different points of the globe. This fact is, that the forces which attract one of the poles of a magnetic needle freely ſuſpended towards the north, are equal to thoſe which attract the oppoſite pole towards the ſouth. Coulomb concludes that this equality exiſts, becauſe a needle, weighed two different times, before and after it was magnetiſed, was found exactly of the ſame weight.

C. Borda has found, by obſervations made firſt at Breſt, Cadiz, Teneriffe, Goree on the coaſt of Africa, and afterwards at Breſt and Guadaloupe, that the intenſity of the force exerciſed by the globe on the magnetic needle, eſtimated according to the number of oſcillations made by the needle in a given time, was ſenſibly the ſame in theſe different places. This obſervation in other latitudes, eſpecially on approaching the poles, might throw ſome light on the theory of natural magnetiſm.—Note of C. Haüy.

the fouth, at different diftances from the poles of the earth, which perform their revolutions in different times; and that, from the combined influence of thefe two poles, we may conclude the annual changes of the declination with fo much precifion, that we can deduce the longitude of any place from its latitude, and from the degree of declination which the needle experiences *.

5. Thus, by fuppofing one or more magnets in the interior part of the earth, the annual changes of the declination and inclination may be explained by the rotary movements of thefe magnets. But in the fyftem, which does not admit thefe internal magnets, it is afked, Whether the changes of declination might not depend on movements which produce the change of obliquity, preceffion, nutation, and perhaps other phenomena or inequalities of that kind †.

6. With regard to diurnal variations, an Englifh philofopher, Mr. Canton, confidering that it is proved by experience that heat diminifhes the force of the magnet, thought that the folar rays, by heating the earth, muft leffen the attractive force of the grand magnet contained in it; and he thence deduced, as will be feen hereafter, an explanation of thefe variations. But Mr. Canton did not reflect on what was

* Heads of Lectures by S. Prieftley, *London* 1794.

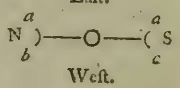
† Æpinus gives another explanation independent of thefe movements. It may be poffible, according to this philofopher, that the declination of the magnetic needle arifes, in general, from the irregular figure of the nucleus of the magnetic globe, or from an unequal diftribution of the fluid in its interior part; and to account for the variation of this declination in one place, in the courfe of time we might fuppofe that the figure of the nucleus, or the diftribution of the fluid it contains, is itfelf variable. Æpinus prefumes alfo, that the action of the iron-mines difperfed throughout the bofom of the globe, may have an influence on the variation in queftion; and may, perhaps, be the fole caufe of it. *Tentamen theoriæ electr. et magnet.* p. 268, 271, 334.

This philofopher wifhes that men of fcience, who have an opportunity of being near a mine of loadftone, would determine, by obfervation, whether the maffes of this mineral, before they are taken from the bowels of the earth, have their poles difpofed, in regard to the poles of the world, like thofe of needles freely fufpended: and whether, in certain maffes, the poles are not in an inverfe direction, of which he fhews the poffibility by means of confequent points. *Ibid.* p. 333.—Note of the fame.

was clearly feen by M. Æpinus, that this magnet, if it exifts, is funk to too great a depth in the earth for the action of the folar rays, or at leaft the variations of that action, in the morning and evening, to be able to penetrate to it. We may, however, apply to the ferruginous minerals, difperfed in abundance over the furface of the earth, what Mr. Canton fuppofed, in regard to the grand magnet contained in its bofom; and then, if we admit that thefe minerals exercife any action on the magnetic needle, we cannot deny that the heat, excited by the rays of the fun, may diminifh that action. From thefe principles it would follow, that in the morning, when the fun warms the furface of the ground fituated to the eaft of the needle, the latter, being lefs ftrongly attracted towards that part, ought to decline towards the weft; and, for a contrary reafon, it muft in the evening decline towards the eaft. But Mr. Canton proved, by a long feries of obfervations, that at London, at leaft, this is the ordinary courfe of the diurnal variations.

7. But it will be proper to examine whether this explanation, even thus corrected, does not contain a paralogifm; and when the attractive force of all the ferruginous particles, difperfed over the furface of the earth, to the eaft of the needle, is diminifhed equally and fimultaneoufly, the needle ought not to remain motionlefs; fince the diminution of the attraction exercifed upon the fouth-pole of the needle, compenfates for the diminution of that exercifed upon the north-pole *.

* Let O be the centre of fufpenfion of the needle N.S., and *a*, *b*, *c*, *d*, the forces which attract the needle in oppofite directions; for example, pieces of iron. The forces in *b* and *d* confpire to make the extremity N of the needle move towards the weft; and the forces at *a* and *c* confpire in the like manner to make the fame extremity proceed towards the eaft. When the needle remains at reft, there is an equilibrium, and the forces $a + c = b + d$. But in this fuppofition, if



the forces of the fame fide, *b* and *c* for example, gradually diminifh, the equilibrium will not be interrupted. For, let $b = y + m$ and $c = z + m$: if the forces *b* and *c* are equally diminifhed by the quantity *m*, we fhall always have $a + z = b + y$. The cafe will be the fame with any augmentation, if it be equal and fimultaneous on all the fides of the needle. —Note of the Author.

I fay

I ſay the ſame thing of thoſe ſituated to the weſt. If this reaſoning be juſt, the needle ought not to vary by the action of the ſolar heat, but when this heat diminiſhes the magnetic force of the ferruginous parts ſituated to the north of the needle, more than that of the parts ſituated to the ſouth, or reciprocally.

To determine this curious queſtion, it would be neceſſary to make choice of two oppoſite coaſts, and directed almoſt eaſt and weſt from the magnetic meridian; ſuch as the coaſt of Provence to the ſouth, and that of Normandy to the north; to eſtabliſh two compaſſes well ſuſpended, ſuch as thoſe of M. Coulomb, one to the ſouth, at Antibes for example, and the other to the north, near Cape de La Hogue; and to ſee whether their diurnal variations did not proceed in a contrary direction: that is to ſay, whether that at Antibes, having the continent to the north, and only ſea to the ſouth, would not decline, in the morning, towards the weſt, as that of Mr. Canton did; and whether that of La Hogue, having the continent to the ſouth and ſea to the north, would not, at the ſame time, decline to the eaſt. Mr. Canton, indeed, who made theſe obſervations at London, had, to the north of his magnetic horizon, the greater part of England and all Ireland; and thus he muſt have had the variation weſt in the morning and eaſt in the evening, as he obſerved it; for it is certain that the ſea preſerves the land, which it covers, from the action of the ſun; and that thus the attraction of that land ought not to vary by the heat which emanates from that luminary.

By carefully repeating and varying theſe obſervations, in places choſen with diſcernment, we ſhall be able to decide whether the regular diurnal variation depends upon a general cauſe, the action of which, however, is ſuſceptible of being ſuſpended or diſturbed by local cauſes; or whether we are to believe, on the contrary, as M. Van Swinden does, that the diurnal variation is not a coſmic phenomenon, or that it does not depend on a general cauſe inherent in the globe, and which every where acts according to the ſame law.

9. Is there properly any action of the magnetic fluid on the electric fluid? or is there, between theſe two fluids, only a reſemblance of properties, or in their manner of action?

10. Is it well afcertained, as M. Van Swinden thinks, that the *aurora borealis* acts on the magnetic needle; and can any idea be formed of the mode of this action?

11. The fame queftion in regard to the zodiacal light.

12. In general, the theory of the magnet is ftill fo far from perfection, even in that part which depends merely on obfervation, that it is much to be wifhed that obfervers and obfervations were multiplied, efpecially in what concerns the inclination of the needle. In regard to the declination and its variations, M. Van Swinden has given a noble example of correctnefs, and of conftancy in obfervations, and in the art of claffing and comparing the refults. It would be of great benefit if this example were followed in different fituations and climates. It would be highly interefting, for inftance, to determine, with precifion, the zones of the earth where the declination is nothing, and where changes take place, and the fame for the inclination.

[To be continued.]

VII. *On the Effects produced on different Kinds of Stones by the Vapour of the Fluor Acid. By* M. KORTUM *of Warfaw* *.

HAVING employed myfelf in engraving on glafs by means of the fluor acid, and having obferved that fome pieces of the glafs I ufed were more fufceptible of being attacked by it than others, I refolved to try the effects of this acid on different kinds of vitreous ftones. Conceiving that the figures would be corroded with more neatnefs, the lefs the filiceous earth contained in the ftones was mixed with foreign matter, I expofed a very bright piece of rock-cryftal, from Swifferland, at a temperature of 18° Reaum. for twenty-four hours, to the vapour of the fluor acid; and found, contrary to my expectation, that no impreffion was made upon it. I repeated the experiment, doubling the temperature and the time of expofure; but the ftone did not lofe any of its fplendor. It is well known that pure rock-cryftal

* From *Voigt's Magazin für den neuesten zustand der naturkunde*, Vol. I. Part 3.

differs

differs from the coloured kinds by this circumſtance—that by expoſure to heat, the former undergoes no change, while the greater part of the latter do. It is equally well known that the action of the fluor acid on glaſs is increaſed by heat.

In order that I might begin a ſeries of comparative experiments in this reſpect, I expoſed, for thirty-ſix hours, at a temperature of 40°, a ruby, ſapphire, leuco-ſapphire, emerald, Oriental garnet, amethyſt, chryſolite, avanturine, giraſol, a Braſilian topaz burnt, a Saxon topaz raw, and an opal; but after being taken from the apparatus, they ſeemed as little injured as the pure tranſparent rock-cryſtal of the firſt experiment.

The diamond, which by its combuſtibility ſhews itſelf to be a peculiar genus, did not ſuffer the leaſt change after four days expoſure to the vapour of the fluor acid; the apparatus being placed on a common German ſtove.

Poliſhed granite, being expoſed to the vapour for three days on a ſtove as before, neither the quartz nor the mica ſeemed to have been attacked. The feld-ſpar, however, attracted my attention; being opaque and muddy, and covered with a white powder. I therefore repeated the experiment on a thin fragment of feld-ſpar of a reddiſh colour, noting its weight, which was 38 grains; and found it to be 2¼ grains lighter. The ſtone had alſo become whitiſh and friable at the ſurface, exactly as when in a natural ſtate of efflorescence.

The different ſpecies of flints, mixed with foreign earths, are more or leſs fuſible. But as this difference is not exactly in proportion to the quantity of the earths mixed with them, the proper explanation of the phenomenon muſt be ſought for in the different degrees of affinity which theſe kinds of earth have for caloric.

Figures traced out on the following ſtones through a covering of wax, after being expoſed for twenty-four hours to the vapour of the fluor acid, the apparatus being placed on the ſtove, were all found to be etched: Chryſopras, Hungarian opal, onyx, Perſian cornelian, agate, chalcedony, green Siberian jaſper, common flint. On the chryſopras the corroſion was above half a line in depth. In thoſe places the

green

green colour of the ftone had difappeared, and the cavities were filled with a white powder. The ftrokes of the figures were expreffed with the greateft finenefs and regularity on the opal; and the lines, in the like manner, were filled with a white powder. The onyx exhibited the contours very clearly, and the etching was pretty deep: the powder with which they were filled was likewife white. On the cornelian the figure was in part etched, and filled with white powder; in part there was only a white efflorefcence, though ftill compact and entire. The agate and chalcedony were, on the other hand, corroded white, but very unequally: here and there cavities were formed, each of which was lined with a white compact fubftance. The green jafper was corroded very unequally, but almoft to as great depth as the chryfopras. Some parts which remained compact, and which were only as it were efflorefced, had loft their green colour and become white.

Flint. The uncovered part, of a bright brown and fomewhat tranfparent fpecimen, had become totally white, but was neverthelefs compact. As I had covered the ftone with wax, leaving a fmall fpot bare, without delineating any regular figure, I obferved that the efflorefcence had begun at the edge of the wax, and proceeded thence to the centre in fuch a manner that the white contour thereby produced refembled an imperfect figure of a fortification, while the inner fpace was only partially efflorefced, and ftill grey, but interfperfed here and there with white points. To free the ftone from the wax, I wafhed it in fpirit of wine. The white figure then gradually difappeared: in half a minute nothing of it was to be feen; and the thin fragment feemed tranfparent as in its native ftate. When it became dry, however, the opaque white figure again made its appearance. After moiftening and drying it feveral times in fucceffion the revival of the natural colour and opacity became always more perfect, and the white efflorefcence remained vifible when wet. As all thefe effects were produced by water and other liquids as well as by fpirit of wine, as I afterwards remarked on other fpecimens, thefe phenomena of the flint had a great fimilarity to the change produced in the colour of the

opal,

opal, and its becoming tranſparent in water. I found means to ſtop the action of the fluor acid on a cornelian and a dark brown jaſper, at that ſtage of the proceſs when the ſtone was ſtill compact, and the colours only whitened. In water both theſe ſtones recovered their natural colour, and when dry became again white.

I expoſed another opaque and almoſt black piece of flint, with effloreſced white points, and the uſual white cruſt, without any covering of wax, at the uſual ſtove-heat of about 16°, to the vapour of the fluor acid for five days in my apparatus. At the end of that period it had loſt almoſt 1-8th of its weight; for it was reduced from 103 to 91 grains, and was thoroughly white, ſo that the ſound parts of the nucleus had entirely the appearance of the effloreſced cruſt. Some parts were friable; and I at the ſame time remarked that the parts of the nucleus, already naturally effloreſced, as well as the cruſt, were much leſs acted upon than the ſound black parts.

It is found alſo, that in regard to the natural efflorescence of flints, the effloreſced cruſt ſerves the ſound nucleus as a cover to defend it, and, by theſe means, retards the total decompoſition of the ſtone. Some, from this cruſt being formed of ſeemingly foreign materials, which is ſometimes conſidered as lime and ſometimes as clay, and which in common reſembles both, have deduced too much, in regard to the formation of flints, when they explain the phenomenon by ſeparation and ſucceſſive hardening from one of theſe kinds of earth. To judge from the above experiments, this cruſt is not properly the matrix, but a reſult from the decompoſition of the ſtone itſelf.

The effloreſcence of feld-ſpar into a granite-like mixture is, with juſtice, conſidered as a very remarkable geological phenomenon, as it announces an endleſs ſeries of the total decompoſition of the preſent combinations. The ſucceſſive degrees of the decompoſition, by means of theſe experiments, may be exhibited with a ſmall and very ſimple apparatus. Turmalin from the iſland of Ceylon, and Zillerthal in Tyrol, green and black columnar ſchorl, and olive-coloured hornblende, after twenty-four hours expoſure, had experienced no change.

White

White Carara marble, in a temperature of 20°, loſt, in twenty-four hours, $\frac{1}{16}$ of its weight; but ſtill the ſhining ſurface of its cryſtalliſed texture was diſtinguiſhable. Weak ſulphureous acid diſſolved one-fourth grain out of forty-five; while, of another fragment, of 18 grains, $1\frac{1}{2}$ grain was diſſolved in the ſame time. Black marble ſuffered no loſs, either in its colour or weight. Agate was not attacked.

Lamellated tranſparent gypſum fell into white powder on the ſurface, after being expoſed for a few hours in the apparatus to the common heat of the ſtove, and the loſs of its weight amounted to $\frac{1}{17}$. This powder was not ſoluble in diluted nitrous acid. From this it appears that the vapour of the fluor acid did not deſtroy the combination of the calcareous earth and the ſulphureous acid, but only abſtracted the gypſum from its water of cryſtalliſation.

As the zeolite, among the ſiliceous ſtones, contains the greateſt quantity of the water of cryſtalliſation, as the gypſum does among the calcareous, I expoſed 102 grains of ſtriated zeolite at the ſtove temperature. In forty-eight hours I found its ſurface friable, and its weight only $85\frac{1}{2}$ grains; conſequently $\frac{1}{6}$ leſs. When immerſed in water, and again dried, it had increaſed $2\frac{1}{2}$. It now weighed 88 grains, but did not recover its ſplendour. On the tin plate, to which the ſtone had been faſtened with wax, I obſerved near the latter a white powder, which had the appearance of ſomething volatiliſed from the ſtone. I poured over the frothy cake of ſparry fluor powder and ſulphurous acid a little water, and, after ſome hours, found the inner ſides of the apparatus of tin plate covered with a beautiful ſilky ſubſtance, of the brightneſs of mother-of-pearl, which was perfectly like that of the zeolite in its natural ſtate. But as I afterwards remarked the ſame appearance of a ſplendour like that of mother-of-pearl, after expoſing other ſtones in the ſame manner, it is not to be aſcribed to the zeolite excluſively. On the contrary, I rather conſider it as an imperfect calcination of the tin; as, by a quicker diſengagement of the fluor acid, it, or a mixture of it with ſulphurous acid, may acquire the property of attacking tin.

Barytes, of a fibrous texture, when expoſed twenty-four

hours in the ſtove-heat, remained unchanged, and had ſuſtained no loſs of its weight or its ſplendour.

That I might not leave magneſian ſtone unexamined, I expoſed, for forty-eight hours, at the ſtove-heat, a thin plate of Venetian talc weighing 124 grains. After the experiment it weighed only 81 grains, and had therefore decreaſed in weight more than $\frac{1}{3}$: it had alſo fallen into a ſoft tender powder, which floated on water, and had the appearance of magneſia. I poured water on the reſiduum in the apparatus, and found next day the ſides incruſted with ſmall cryſtalline glittering flakes, adhering in detached maſſes, which could not be waſhed off with diluted nitrous acid. Though theſe may have been real magneſian ſalt, they ſeem, however, to throw ſome light on the cryſtalliſation of ſtones. Bergman had before obſerved the ſiliceous cryſtalliſation formed in diluted fluor acid, which had ſtood a long time at reſt over powder of ſilex.

Among the various hypotheſes reſpecting the formation of granite, and ſtones in general, that which ſuppoſes the ancient ocean, ſo much ſpoken of by modern geologues, to have conſiſted of a fluid totally different from that of our ſea-water, and of which the latter may be only the reſiduum, is not the moſt improbable. In my experiments, about 50 grains of the powder of ſparry fluor, mixed with as much concentrated ſulphurous acid as was equal to the ſpace occupied by the powder, was put into a tin-plate box, and the latter into another of the content of about 20 ſquare inches; to the lid of which, of ſtrong tin-plate, I faſtened, with wax, the ſpecimens to be tried, and ſhut the whole ſo as to be air-tight. After expoſing various kinds of ſtones for fourteen days I found the inner ſmall veſſel in part corroded, and on the exterior ſides a moiſt, weakly-adhering ſalt, cryſtalliſed in a conſiderable quantity, which at firſt I conſidered as a mixture of iron and fluo-ſulphat of lead, tin being ſeldom worked pure, and the ſolder having begun to give way. It is, however, poſſible that ſome tin may have been diſſolved by the two acids combined. I diſſolved ſome of this vitriol in diſtilled water, and dropped into it ſome ſpiritous tincture of galls. The liquor became of a beautiful indigo-blue colour, inſtead of black or purple as I expected; and a very

light

light precipitate, of the ſame colour, was depoſited at the bottom. A ſolution of muriate of barytes being dropped into the blue liquor, gave a white precipitate, like regenerated barytic ſpar, without changing the blue colour of the firſt light precipitate, which remained like a ſlimy ſubſtance floating on the other, nor the clear liquor: and the latter, after having ſtood ſeveral years, appears as deep a blue as can be produced by ammonia from a ſolution of copper. The blue ſlime was found fit for a pigment, but far inferior to Pruſſian blue.

On this occaſion I recollected the blue colour of lapis lazuli, which Marggraf aſcribes to iron, and in which Rinmann found fluor acid. This ſtone, after two days expoſure, remained unattacked, and its colour unchanged. If, however, it is claſſed among the family of the zeolites, it is, at any rate, of a changed nature; for the latter are eaſily acted upon by the vapour of fluor acid.

According to Bergmann, ſiliceous earth is not ſoluble in pure carbonic acid gas; but it is obſerved in common life, in places where ammoniacal gas as well as carbonic acid gas are diſengaged in abundance; ſuch as dunghills, greenhouſes, priſons, cattle-ſtalls, and ſoap manufactories, &c. that the glaſs in the windows becomes much ſooner opaque than in apartments where that is not the caſe. The cauſe of this is owing in part to a cruſt of foreign matter, and in part to actual corroſion. If it now be admitted that the carbonic acid gas has a conſiderable ſhare in producing the efflorescence of the glaſs in the above-mentioned places, we may aſſume this effect, under certain modifications, in regard to the natural efflorescence of ſtones; as it is not ſo much a ſolution of the earthy baſes, as a ſeparation of the cryſtalline connection.

Though analogical concluſions from chemical reſults are of little value, they have led me to the following conjecture: As the fluor acid hitherto, at leaſt as far as I know, has never yet been analyſed, and as its radical is unknown, I am of opinion that, till ſomething poſitive is learned, we may admit that fluor acid is not eſſentially different from carbonic acid but in the degree of oxygenation, and that they have both one radical, *viz.* carbon.

VIII. *An Easy and Cheap Method of preparing Sal Aëratus, (Carbonat of Pot-ash.)* *By* E. A. HOLYOKE, *M. D. of Salem, Massachusetts* *.

SAL Aëratus, or the salt formed of vegetable alkali saturated by fixed air (carbonic acid), is, on many accounts, so useful, that a communication of an easy method of preparing it at little or no expence may be beneficial. The following may therefore be acceptable, if it be not already commonly known. I have myself prepared this salt for ten or twelve years past in this way, and it is now kept in our apothecaries shops.

Take a large wooden box †, bore eight or ten holes, half an inch in diameter, in the side of it, just below the lower edge of the cover, at nearly equal distances all round; bore also as many holes in the circular bottom of the box, close to the edge of it: then take another box of the same kind, but of a smaller diameter by half or three quarters of an inch; place this in the larger, and, to keep it steady, thrust three or four wooden wedges between the two boxes. The two boxes ‡ being thus prepared, fill the inner one with the purest salt of tartar, or clean well-calcined pearl-ashes, or any clean pure fixed vegetable alkali: put its cover on the outer box, leaving the inner one uncovered; sling this double box, thus filled, with a cord, and suspend it in a distiller's vat or cistern, while the wash is fermenting, a little above the liquor, or in an empty cistern, if it has been much used, and still retains the fixed air: let it remain in this situation for six weeks or two months, or longer if it is not wanted; let it then be taken out, and the salt, now fully saturated with the acid, be exposed to the sun and air to dry.

The salt thus prepared does neither effloresce nor deliquesce

* From the *American Medical Repository*.

† I make use of a common cylindrical box, about nine or ten inches in diameter, and between five and six inches deep.

‡ The design of the outer box is merely to prevent any dust or dirt from getting into the salt, while the holes in it suffer the fixed air to be freely admitted.

in

in the open air, and, for all common purpoſes, is, I believe, equal to that prepared by cryſtalliſation.

Note. The pearl-aſhes had better be put into the box in moderate-ſized lumps than in powder, that the air may have free acceſs to it.

But if any chooſe to have this ſalt in its moſt perfect form, let him proceed in this manner:

Diſſolve as much of the clean vegetable alkali in boiling rain or other pure water as poſſible; filter the ſolution through paper, pour it into a jar of ſtone or earthen-ware, cover the veſſel in ſuch a manner as that the air may have acceſs to it, but ſo as to exclude all duſt or foreign matter. Let it be hung by a cord in a fermenting vat, or ciſtern, for a month or two, in which time a great many cryſtals will be formed; from which the ſuperfluous liquor may be poured off, and the ſalt dried in Hippocrates' ſleeve. The ſuperfluous liquor may be again ſaturated with more alkali, and again expoſed to the air in the ciſtern, without any loſs. This laſt is, without doubt, the moſt perfect mode of preparation, and I have ſometimes made uſe of it; but, as it is much more troubleſome to make than the other, and as the other, for all medicinal purpoſes, is perhaps equal to this, I have for the moſt part employed it.

This ſalt is much more tolerable to the palate, and may be taken in larger doſes than the naked alkali; and as it is decompounded by vegetable acids as well as the mineral, it may be exhibited inſtead of the alkali in perhaps every caſe where the latter is proper, unleſs the fixed air is judged improper.

It is much ſuperior to common alkali in forming Riverius's anti-emetic effervefcing draught, as it contains a much larger proportion of fixed air (in which the principal virtue of that medicine is ſuppoſed to reſide) than the mildeſt fixed alkali, and is at the ſame time much more palatable.

I commonly direct about ʒ ii, or rather more, of this ſalt to be diſſolved in ℥ iii of fair water: a large ſpoonful of this ſolution, added to the ſame quantity of good vinegar, or lemon-juice, at the inſtant of ſwallowing it, makes an agreeable doſe. But the taſte of this ſolution is ſo mild, that, if

the prefcriber choofes, a fpoonful of it may be fwallowed alone firft, and as much vegetable acid immediately upon it, in which cafe none of the gas will be loft.

When acidity abounds in the firft paffages, a little of this falt added to any bitter infufion, or the dry falt added to powder of columbo, or any peptic powder, is an effectual antacid.

In calculous cafes this falt is recommended by writers, particularly by the celebrated Dr. Cullen in his Materia Medica, Vol. II. ch. 13. as being an happy expedient for conveying larger quantities of alkali into the ftomach, than it can bear in its natural ftate.

Hitherto the common mode of preparing the falt for this purpofe, I believe, has been by impregnating a folution of fixed alkali with fixed air, by means of Dr. Nooth's machine; but any one who has prepared the medicine in both ways, will readily give the moft decided preference to that above defcribed, on account both of eafe and cheapnefs.

It is fcarcely worth mentioning, that, for œconomical purpofes, fuch as promoting fermentation in dough for bread or cakes, where pearl-afhes is commonly employed, the fal aëratus is much to be preferred, on account of the much larger quantity of fixed air eliminated in the procefs.

IX. *Communication from* CAPEL LOFFT, *Efq. refpecting the late Meteor and the prefent Comet.*

To the Editor of the Philofophical Magazine.

SIR, *Troflon, near Bury, Suffolk.*

OBSERVING an account of the Meteor of Sunday Sept. 22, in p. 434 of your laft Number, I trouble you with an account of it as obferved here.

I was looking on that evening for the comet, which I expected to have feen between the Northern Crown and the conftellation of Hercules. This being about half paft eight, my eye confequently was then directed weftward. The fky was cloudy and mifty: very few *ftars* appeared. Suddenly my attention

tention was called off from its immediate object by a moſt vivid reflection of light from the clouds of that part of the ſky; as if full day had ſprung on me inſtantaneouſly. I immediately turned round to diſcover whence it proceeded, and ſaw a moſt luminous body, apparently equal (or larger) to the full moon when ſhe appears greateſt, but certainly very much brighter. It was of an exceedingly ſplendid gold colour, and round, except to the weſt, where it was of a ſtrong red, drawing off to purple, and its edge ill defined, and rather unequal. It was about 12 or 15° high, and almoſt exactly in the meridian. It ſeemed nearly ſtationary; but what little motion it had, tending to the horizon nearly at a right angle. In about three or four ſeconds it diſappeared, as if ſinking behind the clouds: I obſerved no ſparkles, nor any luminous train left behind it, nor any exploſion. It was ſeen by many at Bury, and was alſo ſeen at Norwich, and at Cromer on the coaſt north-eaſt of Norwich. Thirty-five minutes paſt eight was the time I minuted of its appearance: but I had not then corrected my watch by an obſervation of the ſun on the meridian, for many days preceding. It might be about eight or ten ſeconds (as I did not find it immediately, the reflection being very widely extended) between my firſt being ſtruck with the reflected light, and the diſappearance of the meteor. Near Norwich it was obſerved to throw out red ſparks, or globules, as in Kent; and was noticed to be of a very white light: and the different colour of its light may be naturally referred to different ſtates of combuſtion, and partly to different ſtrata of atmoſphere through which it paſſed.

I do not at preſent learn it has been ſeen any where much weſtward of the line which theſe ſeveral obſervations indicated. It ſeems to have been very low; and if more obſervations could be collected and compared, it would probably be found to have a very conſiderable parallax, and its altitude and magnitude might be determined, eſpecially if obſervations could be had eaſt and weſt of this line. But it does not ſeem to have been ſeen at London, Peterborough, Oxford, or Lincoln; or even at Cambridge, though ſo very little weſt of the places where it was ſeen. Yet it was ſcarcely

poſſible

poffible to have been not feen by any perfon who was out, if within the limits of the fenfible horizon which circumfcribed it. This feems a ftrong prefumption that it muft have been uncommonly low indeed. Very few ftars being then vifible, it was not eafy to come to much accuracy as to its apparent path.

Meteors have abounded lately. I faw two very brilliant, but fmall ones, in one night, and within three minutes of time of each other; one between the Northern Crown and Bootes, and the other between the Crown and Hercules. The former appeared about the fize and brightnefs of Venus. And on the 2d, at about twenty minutes paft ten, another, which was in the field of my night-telefcope with the comet: befide many fmaller, ufually called *fhooting ftars*.

The laft time the weather has permitted me to fee the comet, was October 4, at twenty-five minutes paft ten, to a very few minutes of its fetting. I thought its nucleus very difcernible, and the extent of its coma rather increafed. From its then appearance I fhould hope, if the weather favours, it will be traced down to its node; which feems likely to be in 17 or 18 of Sagittarius; and that it may ftill be vifible for a fortnight from this time.

I remain yours fincerely,

CAPEL LOFFT.

P. S. It is worthy of inquiry and obfervation, whether nearly all the very large meteors have not been feen in or near the magnetic meridian, as an acute and attentive philofophical obferver thinks to be the fact.

X. *Defcription of a Blaft-Furnace for fmelting Iron from the Ore, with that Part of the Blowing-Machine immediately connected with it. By Mr.* DAVID MUSHET, *of the Clyde Iron-Works. Communicated by the Author.*

FIGURE 1, (Plate I.) reprefents a blaft-furnace, with part of the blowing-machine.

A, the regulating cylinder, eight feet diameter and eight feet

feet high.—B, the floating piston, loaded with weights proportionate to the power of the machine.—C, the valve, by which the air is passed from the pumping cylinder into the regulator: its length 26 inches, and breadth 11 inches.—D, the aperture by which the blast is forced into the furnace. Diameter of this range of pipes 18 inches. The wider these pipes can with conveniency be used, the less is the friction, and the more powerful are the effects of the blast.—E, the blowing or pumping cylinder, six feet diameter, nine feet high: travel of the piston in this cylinder from five to seven feet *per* stroke.—F, the blowing piston, and a view of one of the valves, of which there are sometimes two, and sometimes four, distributed over the surface of the piston. The area of each is proportioned to the number of valves: commonly they are 12 + 16 inches.—G, a pile of solid stone building, on which the regulating cylinder rests, and to which the flanch and tilts of the blowing cylinder are attached.—H, the safety-valve, or cock; by the simple turning of which the blast may be admitted to, or shut off from the furnace, and passed off to a collateral tube on the opposite side.—I, the tuyere, by which the blast enters the furnace. The end of the tapered pipe, which approaches the tuyere, receives small pipes of various diameters, from two to three inches, called *nose-pipes*. These are applied at pleasure, and as the strength and velocity of the blast may require.—K, the bottom of the hearth, two feet square.—L, the top of the hearth, two feet six inches square.—KL, the height of the hearth six feet six inches.—L is also the bottom of the boshes, which here terminate of the same size as the top of the hearth; only the former are round, and the latter square.—M, the top of the boshes, 12 feet diameter and eight feet of perpendicular height.—N, the top of the furnace, at which the materials are charged; commonly three feet diameter.—MN, the internal cavity of the furnace from the top of the boshes upwards, 30 feet high.—NK, total height of the internal parts of the furnace, 44½ feet.—OO, the lining. This is done in the nicest manner with fire-bricks made on purpose, 13 inches long and three inches thick.—PP, a vacancy which is left all round the outside of the first lining, three inches broad, and which is beat full of coke-dust. This space is

 allowed

allowed for any expansion which might take place in consequence of the swelling of the materials by heat when descending to the bottom of the furnace.—QQ, the second lining, similar to the first.—R, a cast iron lintel, on which the bottom of the arch is supported.—RS, the rise of the arch.—ST, height of the arch; on the outside 14 feet, and 18 feet wide.—VV, the extremes of the hearth, ten feet square. This and the bosh-stones are always made from a coarse gritted freestone, whose fracture presents large rounded grains of quartz, connected by means of a cement less pure.

Figure 2 represents the foundation of the furnace, and a full view of the manner in which the false bottom is constructed.

AA, the bottom stones of the hearth. B, stratum of bedding sand. CC, passages by which the vapour, which may be generated from the damps, are passed off. DD, pillars of brick. The letters in the horizontal view, of the same figure, correspond to similar letters in the dotted elevation.

Figure 3, AA, horizontal section of the diameter of the boshes, the lining and vacancy for stuffing at M. C, view of the top of the hearth at L.

Figure 4, vertical side-section of the hearth and boshes; shewing the tymp and dam-stones, and the tymp and dam-plates. *a*, the tymp-stone. *b*, the tymp-plate, which is wedged firmly to the stone, to keep it firm in case of splitting by the great heat.—*c*, dam-stone, which occupies the whole breadth of the bottom of the hearth, excepting about six inches, which, when the furnace is at work, is filled every cast with strong sand. This stone is surmounted by an iron plate of considerable thickness, and of a peculiar shape *d*, and from this called the dam-plate. The top of the dam-stone and plate is two, three, or four inches under the level of the tuyere hole. The space betwixt the bottom of the tymp and the dotted line is also rammed full of strong sand, and sometimes fire-clay. This is called the tymp-stopping, and prevents any part of the blast from being unnecessarily expended.

The square of the base of this blast-furnace is 38 feet; the extreme height from the false bottom to the top of the crater is 55 feet.

XI. *Extract of a first Memoir to serve as a Natural, Chemical, and Medical History of Human Urine; with some New Facts on its Analysis and Spontaneous Alteration. By* C. FOURCROY *and* VAUQUELIN *.

THERE is no animal matter which has been subjected to more examination than urine, and there is none which has furnished more discoveries to chemists. They have, however, confined themselves chiefly to an examination of phosphats, which were for a long time called Fusible Salts. Margraff, Pott, Schlosser, Haupt, and Rouelle the younger, were almost exclusively occupied with them from the time of Boerhaave to Scheele. This great attention bestowed on phosphats arose from the interest inspired by the discovery of the phosphorus of urine, and the ideas which the alchemists had propagated respecting the singular properties of these salts. What physicians have done in regard to human urine has scarcely had any relation to its nature, and has supplied too much to the ridiculous pretensions of empiricism. The discoveries of Scheele put an end to the incoherence between medical observations and the chemical labours on urine. When the acid, which forms the greater part of the urinary calculi, or the uric acid, was discovered, as well as the acid phosphat of lime, the formation of these calculi, like those of the depots of the precipitates of urine, became much easier to be comprehended than before; and it then became possible to establish between the medical observations on urine and its nature, better ascertained, that relation which ought constantly to have been the physician's guide, since by it alone he could be furnished with any exact light. The useful inferences which our colleague Berthollet has drawn from the examination of urine, and from its nature, more or less acid, in gouty affections, are at present well known.

A long series of experiments, undertaken by C. Vauquelin and myself, on animal matters in general, presented to us

* From the *Annales de Chimie*, No. 91.

 long

long ago a number of new facts in regard to the urine of men and animals. Our new analysis of urinary calculi induced us to resume, in greater detail, an examination of human urine, the natural source of these concretions. The result of our labour on this liquor we communicated to the Institute in the sitting of the 11th of Frimaire this year; and I shall here give a short account of it, sufficient to make it known, and to prove how much light may be thrown, by an analysis of this kind, on the physical nature of animals.

I. In this memoir the smell of human urine is first considered as a very distinct and characteristic property of this liquid. In urine well constituted, and when it issues from the bladder, it is neither the odour of ammonia, nor of an acid, nor of the violet: it is evidently aromatic, and depends entirely on a matter peculiar to urine, which makes it to be what it is, and without which it would not be urine.

II. The orange colour of urine is, no less than its odour, a property which exclusively belongs to it, and which is found in no other animal liquid. Being susceptible of many shades, and exceedingly varied degrees in its intensity, as was long ago observed by Bellini, it is indebted for this variation to the very variable proportion in the effect of the water and the colouring matter, and the latter is the same that gives to this liquid its aromatic odour. The darkest coloured urine, either naturally or by artificial evaporation, assumes all possible shades, like those remarked under different circumstances, by the addition of water only in various quantities. Thus the smelling and colouring matter of urine is very soluble in water.

III. The authors of the memoir, in treating of the acrid and strong taste of human urine, remark that this acridity is not merely that of the saline substances held in solution in this liquor, which only modify it, by giving it a saline taste. The durable acridity of urine depends also on the matter which produces its smell and colour. It varies, therefore, like these two properties, which it follows in their intensity or diminution. When physiologists ascribed the savour of urine to these salts, they did not know, or did not

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pay attention to the ſmall quantity of them in proportion to the maſs of water in which they are diſſolved.

IV. The component materials of urine, the number of which is conſiderable, re-act on each other during the proceſs of analyſing them. The ſalts contained in it are modified, and change their nature: but the property, above all, of becoming alcaliſed, or of forming, by its ſpontaneous alteration, ammonia and carbonic acid, (a property which is developed in a few moments in an elevated temperature, and which has made it be conſidered as the moſt alcaleſcent of all the animal humours,) is the ſource of the moſt ſingular changes in its nature. Inſtead of remaining acid, it then turns vegetable colours green; it produces an effervefcence with acids when poured over it; it changes its colour; it aſſumes a fetid ammoniacal ſmell; it depoſits precipitates and cryſtalliſed ſalts which it did not contain. This alteration begins ſometimes even in the reins, and carries with it a diſpoſition to form calculi, which it would not have formed without it. It depends entirely on the urinary matter, the cauſe of its odour, its colour, and its ſavour.

V. It follows, from the preceding conſideration, that the analyſis of urine, by the means at preſent employed, muſt have given many uncertain reſults; and that many errors, in this reſpect, muſt have been committed. The action of fire, which ſo ſpeedily and ſo eaſily alters the nature of bodies, changes both the proportions and the properties of its productions. Urine, therefore, muſt be examined at the moment when it iſſues from the body; the component parts of it muſt, as much as poſſible, be ſought for without employing fire; in examining it, re-agents ought to be uſed, which, as in the analyſis of mineral waters, may ſerve to aſcertain, at the moment of their mixture, the matters contained in that liquid. The phoſphoric, the uric, and the muriatic acids, with lime and ammonia, have already been ſhewn in it; but ſcience is as yet far from being in that advanced ſtate as to poſſeſs the number of re-agents neceſſary for this kind of analyſis, which ſtill requires many new reſearches.

VI. Our reſearches on the means of analyſis, carried much farther

farther than any before undertaken, have been attended with no other fuccefs than to enable us to compare the phenomena they exhibited with thofe of evaporation. Thus the tranfparency of turbid urine, and the concrete flakes precipitated during the evaporation of urine, have fhewn us that the cauftic alkalies, and the precipitate formed by the tanning principle in the liquid, belonged to the phofphat of lime and a gelatinous animal matter. We have learned, by the fame comparative procefs, that even a very gentle heat formed in urine ammonia, which fpeedily neutralifed its acidity: that its colour embrowned by evaporation, and its abundant cryftallifation, on cooling, after it had been brought to the confiftence of fyrup, depended on the concentration of the particular matter, the common fource of its colour, odour, favour, and its other characteriftic properties: that the fetid garlic fmell, and the cryftalline form, were two of its moft prominent characters: and, in the laft place, that as all urine evaporated in this manner forms itfelf into a mafs, there ought to be found in this mafs the conftituent matters of urine, except the portion of ammonia formed and volatilifed by the evaporating heat. This lamellated cryftalline mafs, treated with alcohol, was almoft entirely diffolved, and nothing remained but a little of the grey faline fubftance which the water feparated from the phofphat of foda and ammonia, and a little phofphat of lime and uric acid, infoluble in the liquid, but which was infulated from the calcareous phofphat by the ley of cauftic alkali. Thefe falts, and this acid, made fome milliemes only of the weight of the urine; while the matter diffolved by the alcohol formed fome centiemes. The latter was compofed of a little muriat of ammonia, benzoic acid, and urinary matter more abundant than all the reft. Such is the feries of our analytical proceffes, analogous to thofe employed on the refidues of mineral waters, and by the help of which we have been able to feparate the conftituent matters of human urine more exactly than had been done before.

VII. The diftillation of urine, though confidered as well known, prefented to us feveral remarkable facts. In a very gentle fand-bath, frefh urine gives water very ammoniacal and

and cryſtalliſed carbonat of ammonia long before it is dry: the laſt portions of water obtained effervefce ſtrongly with acids, and become of a roſe-colour, not much ſuſceptible of changing in the open air. It ariſes from the carbonat of ammonia which the liquid product contains; and urine has a ſingular diſpoſition to form this ſalt in great abundance and with great eaſe, as is proved by all the means of analyſis applied to it. The native acid of the urine is then ſaturated; flakes of animal matter are depoſited, as well as earthy phoſphats and the uric acid. All theſe phenomena continue until the predominance of the carbonat of ammonia, which is formed, becomes very manifeſt. They take place at a continued temperature of 60°.

VIII. We never ſaw, with any exactneſs, but three facts in regard to the putrefaction, or ſpontaneous and ſeptic decompoſition of urine, *viz.* the horrible fetor by which it is accompanied; the formation of a great quantity of ammonia which characteriſes it; and the abundance as well as eaſy extraction of the fuſible ſalts or alkaline phoſphat which follows. C. Hallé has given an exceeding good deſcription of the ſucceſſive alterations which urine, left to itſelf, experiences, but he has not followed them in regard to the nature of their materials; the object of his reſearch was only to deſcribe the effects in their appearances. Urine contained in a cloſe veſſel becomes of a darker colour; turns brown, and even black; emits a fetid, ammoniacal odour; depoſits firſt a light cloud, which is gradually changed into mucous flakes more or leſs coloured. There are formed at its ſurface, or on the blackened cruſt by which it is covered, and on the ſides of the veſſel which contains it, cryſtals in needles or in regular priſms, or ſilky tufts. Urine is then ammoniacal inſtead of being acid. Diſtilled to a half, it gives a great deal of the fetid carbonat of ammonia: being carried ſtill farther, ſo as to become ſyrupy, it furniſhes acetite of ammonia: the thick part gives, by the addition of acids, a ſharp and acetous odour. This reſiduum of urine, when putrid and evaporated, does not, by the addition of the concentrated nitric acid, preſent thoſe white concrete and abundant cryſtals which ariſe in freſh urine evaporated to the ſame degree,

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and which belong to the urinary matter not changed. It was this matter which experienced the greateſt and moſt ſingular change by putrefaction; it was the focus and ſubject of it; and it gave birth, above all, to the carbonat of ammonia which replaced it, and to the acetous acid, which, as the phoſphoric and the uric, is found ſaturated by this kind of alcali. It appears, therefore, that in order to procure more of the phoſphat of ammonia, it is of advantage to employ putrid urine.

IX. An attentive and careful examination of the firſt phenomena of the putrid decompoſition of human urine, preſented to us reſults as intereſting at leaſt as that of urine entirely decompoſed. Theſe reſults are connected, above all, with the formation of urinary calculi, which were one of the moſt important objects of our labour. We were deſirous of adding to the well-known fact of the exiſtence of calculous matter in every kind of urine, anſwers to the following queſtions of ſo much importance:—Why are theſe matters more abundant? Why does a diſpoſition to calculus exiſt in ſome ſubjects and not in all, though the urine of all contains what may form them? Why are they formed ſometimes ſpeedily, and ſometimes ſlowly? What is the cauſe of the ſix-fold variety of the calculous matters, their mixture, and interruption? Though we were far from having found a ſolution to all theſe queſtions, we have at any rate been able to reſolve ſome.

The white priſmatic cryſtals, which are depoſited on the pellicles or the ſides of veſſels containing urine, do not ſhew themſelves till the liquid becomes ammoniacal. They increaſe in quantity and bulk for ſix or eight days: they are priſms of ſix planes, with pyramids having ſix faces, which are readily diſcovered to be ammoniaco-magneſian phoſphat, like that often found, under the ſparry form, at the ſurface of white calculi. This ſalt does not exiſt, or is not formed, but when there is an exceſs of ammonia in the urinary liquor. This ammonia, by ſaturating the free phoſphoric acid, ſeparates the gelatinous matter which gives birth to a mucous precipitate, and conſtitutes with that acid, and by uniting itſelf to the phoſphat of magneſia, the ammoniaco-magneſian

phoſphat, which depoſits itſelf in cryſtals. The uric acid is equally ſaturated, and preſents urate of ammonia; which ſometimes depoſits itſelf in the calculous matter with the triple phoſphat juſt mentioned. This triple phoſphat ſeparates itſelf in cryſtals of the liquor only becauſe it is leſs ſoluble than the two inſulated phoſphats. This ſalt, therefore, which does not exiſt quite formed in the urine, is the produce of its ammoniacal alteration.

As ammonia and the carbonic acid go on always increaſing when the uric and phoſphoric acids and the phoſphat of magneſia are ſaturated, there remains in the liquor carbonate of ammonia, which then renders ſyrup of violets green, and effervesces with acids. The acetous acid, which is formed at the ſame time, becomes ſaturated alſo with ammonia; ſo that the urine contains, at the ſame time, acetite and carbonate of ammonia. Theſe three ſubſtances, formed almoſt at the ſame time by the decompoſition of the urine; ammonia, the phoſphoric and the acetous acids, are the produce of the urinary matter, which ceaſes to be found in that liquid when altered by putrefaction.

X. This alteration of urine does not always take place in the ſame manner, and its decompoſition varies according to the diverſified nature of that liquor. Sometimes, in one individual, when the urine commonly preſents the before-mentioned phenomena, the liquid, inſtead of emitting an ammoniacal odour, becomes covered with a green and white mouldineſs, which increaſes for fifteen or twenty days. Inſtead of containing ammonia naked, it contains the acid, and emits a ſmell of it. This kind of urine is leſs ſubject to alteration than the preceding. It appears to be at leaſt as common as that which alcaliſes. Their difference depends evidently on the variation of their principles, and not on their nature; for they are almoſt always the ſame, except in their proportion. Thus the urinary matter, the ſource of the formation of ammonia, of the carbonic and acetous acids, and the cauſe of the alterability of urine, does not produce or experience that alteration, or that decompoſition, but ſo far as it is mixed with a certain quantity of gelatinous matter, which ſerves it as a ferment. If it does not contain

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enough, and if the urine, lefs gelatinous, is by thefe means more coloured, has a ftronger fmell, and is more charged with urinary matter, it is lefs fufceptible of fermentation or putridity; it preferves itfelf much longer and with its primitive characters, and is more permanent: that, on the other hand, which is lefs coloured, more changeable, and more difpofed to the formation of ammonia, depofits fpeedily mucous flakes, and gives fooner a cloud and precipitate. It appeared to us, that urine lefs corruptible and lefs gelatinous, and in fome meafure more urinous, if I may be allowed the expreffion, was the fign of good health, and the produce of complete digeftion; while pale urine, more gelatinous and more decompofable, exifted more particularly in weak subjects, and in cafes where the digeftive faculties were leffened. There is reafon to believe that thefe two different ftates of urine, which exhibit it as two diftinct or different liquids, will one day furnifh facts of great utility to the healing art; and that a folution of tan will fupply the means of diftinguifhing them, and of determining their relation in regard to the nature and quantity of the precipitate which it will form in thefe liquids, compared in different fubjects, or in the fame at different periods.

XI. Scheele firft announced the prefence of the benzoic acid in the urine of man, and particularly in that of children. Rouelle the younger had before fufpected it in that of the mammiferæ, though he durft not give any opinion as to its nature. This acid is obtained by fublimation, on heating the extract of urine in clofe veffels. It may be feparated ftill better by evaporating urine to the confiftence of clear fyrup, and pouring on it the muriatic acid, by which it is precipitated; becaufe that acid decompofes the benzoat of ammonia that has been formed. It is by the latter procefs that we have taught the method of extracting it from the urine of horfes and cows, and, above all, from the water of dunghills, in fufficient abundance to fubftitute it in pharmacy for that of benzoin. In a word, this acid is the leaft abundant, and perhaps the moft variable of the matters in urine. It appeared to us to be between $\frac{1}{1000}$ and $\frac{1}{10000}$. There are fome morbific circumftances under which its quantity

increafes

increafes very fenfibly, and often very fpeedily. We had no opportunity of determining the difference of the proportion in the urine of adults, or that of children, in which Scheele fays that it is much more abundant.

XII. The analyfis of urinary calculi, which firft directed our attention to urine as the fource of its concretions, induced us to examine whether the oxalic acid exifted in this liquid. The oxalat of lime is, indeed, one of the moft frequent matters of calculi; and we have found it in the proportion of a fixth in the number of urinary ftones we examined. None of the means which can be employed to difcover the prefence of that acid, exhibited it to us in urine; while, on the other hand, the fmalleft quantity of the oxalic acid, which we poured into the liquid, gave us an abundant and very heavy oxalat of lime; which proved to us, that fuch an acid could not remain diffolved in the urine. Thus, when a mural or mulberry-formed calculous, compofed of the oxalat of lime and an animal matter, by which it is agglutinated, arifes in the urinary duct, its production takes place even at the moment of the formation of the oxalic acid. This acid muft arife in the urine; and in that cafe there muft be in it an unnatural and morbific foreign production. There is reafon to believe that fome kinds of urine, which come from the body white and turbid, are charged with this falt; and that the oxalat of lime, formed by a caufe ftill unknown, iffues in this manner without producing calculi. It may be thence feen of how much importance it is to make a chemical examination of the urine of difeafed perfons, and to eftablifh a continued feries of experiments on this fubject in fome hofpital fet apart for thefe ufeful refearches.

The cafe is the fame with filiceous earth as with the oxalat of lime. We have never yet found it in urine. And its exiftence in that liquid appears to be a rare cafe in pathology: of three hundred calculi, indeed, analyfed with care, two only were found containing filex.

XIII. This analyfis fhews that feveral matters, hitherto unknown in urine, exift in it, *viz.* 1. Phofphat of magnefia. 2. The urat of ammonia, which is formed at the time of the decompofition of the urine. 3. Albumen and gelatinous

matter. 4. The oxalic acid, which is produced under some morbific circumstances. 5. Siliceous earth, which is found in it only very rarely. The four first substances are constant in it; the other two are only accidental, and therefore we pass them over till after the examination of calculi arising necessarily from urine, and of which the constituent matters have been dissolved in that liquid. Another new fact obtained by our labour is, that the particular matter which gives to urine its distinguishing characters, is converted, by means of the fermentation excited by the presence of the gelatinous body, into ammonia, carbonic acid, and acetous acid, and from these new products there arise in urine very remarkable changes. The analysis of putrid urine differs, therefore, very much from that of fresh urine.

It results from these researches, by which human urine has become much better known than it was before, that it contains ten principal or constant substances, characterised in the following manner;

A. Muriat of soda, which crystallises in octaedra in evaporated urine.

B. Muriat of ammonia; the natural octaedral form of which is modified into a cube by its union with the particular urinary matter like that of the preceding, and passes through the same combination from the cube to the octaedron.

C. The acid phosphat of lime, forming about the 700th part of urine, considered formerly as an earth, precipitating itself by the addition of alkalies, which take from it its excess of acid; carrying always with it a gelatinous matter which this acid held in solution, and rendering urine turbid at the moment when the ammonia is disengaged.

D. Phosphat of magnesia, decomposed by alkalies, and giving its earth mixed with the phosphat of lime which is deposited; becoming a triple salt, and separating itself in crystals by the spontaneous formation of ammonia.

E. The phosphat of soda, efflorescing in the air, and always united to the phosphat of ammonia.

F. The phosphat of ammonia, little abundant when the urine is fresh; increasing much by decomposition and the formation

tion of ammonia, and giving only phoſphorus when the whole fuſible ſalt of urine is heated with carbon.

G. The uric acid, named very improperly at firſt the *lithic acid.* It cryſtalliſes by the cooling of urine, and forms the red ſand which that liquid depoſits at the bottom of pots. It is more abundant in the urine of diſeaſed perſons. It may be diſſolved exceedingly well by the cauſtic alkali.

H. The benzoic acid, more abundant in the urine of children; eaſy to be obtained from urine evaporated to a ſyrup, mixed to a tenth with concentrated ſulphuric acid, and diſtilled.

I. Gelatinous matter and albumen, extremely variable in its proportion in the different kinds of urine; ſhewing itſelf in clouds in urine where ammonia is formed, in filaments in that over which alkali is poured, in flakes by the evaporation of urine; precipitating itſelf by the tanning principle, which ſerves to determine the proportion; occaſioning a ſpeedy putrefaction of urine, which contains it in abundance; appearing by their augmentation to be the firſt cauſe of the formation of calculi, and furniſhing them with the gluten which unites their moleculæ, and following, in their proportion, the energy or weakneſs of the digeſtive forces of the diſtribution of the chilous matter.

K. The ſpecial urinary matter, giving to urine its characteriſtic properties really conſtituting it; giving it its odour, colour, and ſavour—its alterability into ammonia, the carbonic and acetous acids, &c. It is the moſt abundant of the matters contained in urine; it forms alone the $\frac{19}{20}$ of its matters. It has been improperly conſidered by Rouelle the younger as a ſaponaceous extract. To it is owing the almoſt total cryſtalliſation of urine evaporated to the conſiſtence of ſyrup; the ſolid and cryſtalline form it aſſumes in that ſtate by the addition of concentrated nitric acid; the cryſtalliſation of the muriates of ſoda and ammonia modified, and in ſome meaſure reverſed—the former from the cube to the octaedron, and the ſecond from the octaedron to the cube. This particular animal matter, which we call *urée*, will form the object of another Memoir, deſtined to ſerve as a ſupplement to

the prefent. It is of much importance to animal phyfiology and the art of healing.

It appears that, befides thefe ten fubftances, the real and conftant materials of human urine, it contains fometimes, but rarely and accidentally, fulphat of foda, muriat of potafh, fulphat of lime, oxalat of lime and of filex: that fome of thefe fubftances, and particularly the two latter, are only the rare productions of fome particular and perhaps morbific difpofitions of urine. It will be of great utility to inquire into the circumftances which have an influence on the exiftence of thefe matters, which are foreign to the natural ftate.

XIV. In characterifing urine as a liquid very diftinct from all others, the matter which I have called *urée* gives it, above all, the property of becoming, by the decompofition which it occafions, a liquor very different from what it was when it iffued from the bladder, and a fubftance totally new. Fermented urine is changed in almoft all its matters. The view of thefe changes, which terminates this Memoir, exhibits, as the moft ftriking refult, the production and exiftence in putrid urine of nine new matters, which do not exift in frefh or natural urine.

1. Ammonia in excefs.
2. The phofphoric acid faturated by this alkali.
3. The phofphat of magnefia converted into ammoniaco-magnefian phofphat.
4. The urat of ammonia.
5. The acetous acid united to ammonia.
6. The benzoic acid faturated with the fame ammonia.
7. The muriat of foda become octaedral.
8. The muriat of ammonia become cubic.
9. The carbonat of ammonia.

We may ftill add the precipitation of the albumen and gelatinous matter effected by the ammonia, and which accompanies that of the phofphats; fo that thefe falts, like the matter of bone, are fufceptible of giving carbon when heated.

Such is the general view of the facts contained in this firft Memoir. They prove how many new and important refults

a pro-

a profound ſtudy of urine may preſent to thoſe who will beſtow on it that continued attention which it requires, and what influence ſuch phenomena may have on the progreſs of animal phyſiology. The authors promiſe to make known, in a ſecond Memoir, all the properties which diſtinguiſh the particular matter that characteriſes this liquid, which is the moſt abundant in it, and which they call *urée*, as has been already ſaid.

XII. *Extract from the Report reſpecting Experiments made at the Polytechnic School in the Years V. and VI. on the Combuſtion of the Diamond.* *By* C. GUYTON *.

THAT the diamond is combuſtible, is a truth which Newton, in ſome meaſure, conjectured; which experience has fully confirmed; and reſpecting which it is no longer poſſible to entertain the leaſt doubt. But what is the nature of this combuſtibility? My firſt experiments, publiſhed in 1785, on the entire combuſtion in nitre in fuſion †, ſeemed to announce that the diamond burnt in it after the manner of coal, ſince it left an efferveſcent alkali; and this ſuſpicion acquired more reality, after the examination made by Lavoiſier of the gas remaining in the veſſels in which it had burnt, and which he found charged with carbonic acid. Mr. Tennant has ſince furniſhed us with a new proof of this important fact, by repeating the combuſtion of the diamond by nitre in a gold crucible, as I had propoſed, in order to obtain a reſiduum abſolutely free from all foreign matter ‡.

There were, however, ſtill ſufficient reaſons to induce us to diſbelieve that the diamond and carbon, or that the diamond and the carbon extracted from the carbonic acid by the noble experiment of Mr. Tennant, were the ſame ſubſtance. Independently of their external characters, ſo completely different, ſeveral obſervations, which I have already communicated to the Inſtitute, prove that their chemical characters

* From the *Annales de Chimie*, No. 91.

† *Opuſcles de Bergman*, French edition, Vol. XII. p. 124.

‡ Philoſophical Tranſactions 1797.

no

no less excluded this identity. Indeed if the diamond was pure carbon, why had it not the same affinities? Why does it not, like it, serve to make the oxigenated muriat of potash detonate; to deoxygenate sulphur, arsenic, and phosphorus; to deoxydate metals, which are sufficiently fixed to undergo the degree of fire which determines its combination with oxygen? Why does it not form also carbures? Why does it not, like it, conduct the electric fluid? We know that the aggregation conceals sometimes the affinities by counterbalancing their power, but not in operations where the bodies are sufficiently fixed, and the temperature sufficiently high, to render effectual the weakest attractions. Something remained, then, to be discovered to harmonise and make us comprehend facts in appearance so contrary *. I imagined that it was by attentive observation of what took place during the act of the combustion of the diamond, that we should attempt to penetrate this secret of nature. The experiments I am about to describe will, I hope, prove that my attempt has not been vain; that the explanation of phenomena, which have appeared to us the most incoherent, may hereafter be deduced from some circumstances which were not observed nor even suspected, and which have enabled us to make an important step in the knowledge of the nature of the diamond, since we can indicate substances which approach much nearer to it than carbon.

These experiments, begun in Thermidor year 5, were not terminated till the 11th of Fructidor year 6. Various accidents, which may be readily conceived, and the fewness of the days when the rays of the sun are not interrupted by clouds or weakened by vapours, were the principal causes of this delay. I shall suppress the details of those essays which did not lead to conclusive results; but I shall not neglect those which furnished us with an opportunity of observing several times the same facts, and sometimes in a more distinct manner; though, by the effect of some unforeseen circumstance, it was not possible to keep an account of them in

* C. Berthollet, in his Lectures at the Normal School, still leaves it in doubt whether the diamond is crystallised carbon, or carbon combined with some other substance.

regard

regard to the correſpondence of the ingredients and the products. I muſt not omit to remark, that I had, as co-operators in theſe experiments, C. Clouet and Hachette. The journal of them was correctly kept by C. Deſormes, formerly a pupil of the Polytechnic School, and at preſent aſſiſtant-preparer for one of the courſes of chemiſtry. The moſt important phenomena were ſeen, at different ſittings, by ſeveral men of letters.

The Council of the Adminiſtration of the Polytechnic School, approving the object of the experiments, authoriſed me to diſpoſe of ſome of the diamonds in its cabinet *.

The firſt experiment was made on the 9th of Fructidor in the year 5. We placed upon the table of the mercurial pneumatic tub, a bell of flint glaſs 18·3 centimetres in diameter, and of the content of 5580 cubic centimetres. Near the tub was placed, on one ſide, a pneumatic machine, to exhauſt the common air from the bell by means of a bent tube, which roſe as far as the knob. On the other ſide was a water pneumatic tub, bearing a large receiver, having at its tubulure a cock which communicated with the inſide of the bell by a tube of bent glaſs, and riſing, in the like manner, to the hollow knob of the bell. On one of the edges of the mercurial tub was fixed a ſlide, bearing a kind of mandril, deſtined to receive a cylinder of hard wood moveable in every direction, terminated by a handle of iron, and ſerving to ſupport a cup made of the crucible earth of Valogne; ſo that this cup could be conveyed to every point of the interior part of the bell to preſent the diamond to the focus. This apparatus is repreſented by fig. 1. (Plate II.) Every thing being diſpoſed in this manner, we put into the cup

* The diamonds, which form part of its collection of minerals, were found in an Engliſh ſhip from Senegal, captured in the year 2. They were depoſited at the *Hotel des monnoies*, where they remained till the year 5, when, on the ſuggeſtion of our colleague Mongez, one of the adminiſtrators, the miniſter of the finances, after cauſing it to be aſcertained that the greater part of them were unfit for being cut, and for that reaſon more valuable for inſtruction, ordered one third of them to be given to the Muſeum of Natural Hiſtory, another to the Cabinet of the School of Mines, and another to that of the Polytechnic School. The lot aſſigned to the laſt conſiſted of 26, weighing altogether 3·662 grammes.

an incomplete octaedral diamond, having the edges a little rounded, of a dirty water inclining to yellowish-grey, and weighing exactly 142 milligrammes. On the cup, the edge of which was ground flat, a cover was applied, attached to a thread tied round the lower part of the support.

The pump was made to act till the mercury in the bell rose within less than a millimetre of the orifice of the tubes of communication. The cock of the receiver of the hydro-pneumatic tub, which had been previously filled with oxygen gas obtained from the oxygenated muriat of pot-ash, was then opened; and the first portion of this gas which had passed into the bell was extracted by the pump, in order to exhaust as much as possible the remainder of common air. After this it was filled with the same gas to within 69 millimetres of its internal edge, and 51 of the external. It may readily be conceived that still a little air remained in the cup in which the diamond was placed, and which, during these experiments, had been shut by its cover; but its content not being altogether three cubic centimetres, this inconvenience was thought the smallest of those which were to be apprehended.

The diamond having been uncovered, we began, at ten minutes after one, to throw upon it the focus of the large lens belonging to the cabinet of the Polytechnic School, the diameter of which is 40·59 centimetres, and the focus 135·3. We were well aware of the necessity of heating the bell by degrees, to prevent its cracking. For this purpose we interposed, at first, green and blue coloured glass; but whether they acquired more heat, or resisted dilatation more, these glasses all speedily broke; and we were not able to accomplish our object but by covering with paper, for some moments, the part of the bell which received the luminous disk. When the paper was withdrawn, the mercury fell rapidly, 19 millimetres in the inside. The diamond, exposed to the focus for twenty minutes, did not inflame. It appeared at first mealy, but sensibly blackened at the surface when observed through the coloured glass while exposed to the focus *.

The

* This carbonaceous colour had been before observed by Lavoisier in his

The focus having been intercepted by an opaque body to examine more cloſely the ſtate of the diamond, no alteration was remarked in it, except that it had aſſumed a yellowiſh ſhade, perfectly like that of tranſparent amber. The atmoſphere beginning to become charged with vapours, the experiment was diſcontinued, in order that we might reſume it at a more favourable moment. This moment occurred the next day the 10th, and was announced by a riſing of the thermometers; one of which, expoſed to the ſun in the open air, roſe to 40 degrees; and the other, expoſed to the ſun alſo under a bell-glaſs to compare the interior temperature, roſe to 44 degrees.

The focus was thrown upon the diamond at 28 minutes after 11. At 42 minutes after 11 the cone of light was intercepted, and we ſaw the diamond red, tranſparent, and ſurrounded with a faint radiation. When cooled, its edges appeared blunted: we obſerved in it a black point; but it had become white, and had loſt the yellow colour acquired the day before.

The experiment could not be reſumed till the 15th. We began, by noting the height of the interior column of the mercury, to calculate, according to the temperature and preſſure, the volume of the aëriform fluid remaining under the bell; and we judged that it had decreaſed about 173 cubic centimetres.

The rays of the ſun were bright and ſtrong; but the air ſo much agitated, that while the thermometer under the bell was at 44·5, that expoſed to the ſun in the open air did not riſe higher than 32°. There was a moment, however, when the luminous cone produced a ſlight ſcintillation on the ſurface of the diamond. An opaque body, immediately interpoſed, made it appear red; but more obſcure than on the 10th. It was alſo found white after cooling.

Being aſtoniſhed that the diamond, when inflamed, as on the 10th, did not maintain of itſelf the temperature neceſſary for its combuſtion, eſpecially in oxygen gas, as happens to

his experiments made on the diamond with the large lens of Trudaine. See *Dict. de Chemie de l'Encyclop. Method.* Vol. I. p. 741.

metallic combuftibles*, we imagined it might refult from its being too much in a mafs, or perhaps alfo too much infulated from every other combuftible which might contribute to this temperature: and that we might make an attempt to remove this obftacle, we introduced into the fame porcelain cup, and without deranging the apparatus, a fmall cut diamond of the weight of eight milligrammes; but there was no appearance that the combuftion was in the leaft augmented; and this fmall brilliant, inftead of being more rapidly attacked by the heat, after having been two days expofed to the action of the folar fire, capable of igniting obfcurely the large diamond placed clofe to it, gave no figns of inflammation, and was taken from the apparatus without having experienced the flighteft alteration, either in the polifh of its furface, or the vivacity of its edges.

On the 23d of the fame month we took the diamonds from the faucer, to examine, with care, that which had given manifeft figns of a commencement of combuftion. It weighed no more than 88 milligrammes; it had therefore loft 54, about 0·38 of its weight. It ftill retained its original octaedral form; but the angles were blunted, and the edges rounded. The furface was tarnifhed, and full of fmall inequalities; which, obferved with a magnifying glafs, prefented cavities, falient points, and fometimes parallel fections of the laminæ. In feveral of the cavities we could plainly perceive a fort of fpecks inclining to grey; but what appeared worthy of moft attention was, a pretty large hollow almoft at the extremity of one of the quadrangular pyramids, which feemed to indicate the place where the folar focus had exercifed, at the end of the operation, its greateft intenfity; and where we diftinguifhed a blackifh ftripe, not terminated like a ftroke formed by a foreign body, but on the contrary foftening itfelf off, and penetrating into the interior part of the mafs by degrading its colour.

* We had the more reafon to be furprifed at this phenomenon, as M. Landriani had announced that the diamond, inflamed by brafs wire, burnt like it in oxygen gas, and with the utmoft brightnefs: he, indeed, excepted the Brafilian diamonds, which he was not able to inflame by thefe means. *Annales de Chemie*, Vol. XI. p. 156.

I thought

I thought it might be of ſome importance to preſerve the ſubject of theſe obſervations, and that it would not diſgrace the collection of the cabinet of the School, with an inſcription alluſive to the experiment to which it had been ſubjected. Another diamond was therefore deſtined to be put into the apparatus, in order that it might be there ſubjected to entire combuſtion. This diamond was alſo a pretty regular octaedron, of a much more beautiful water than the preceding, and weighing 200·1 milligrammes, 3·77 grains.

As the ſeaſon was already ſo far advanced that it left us no hope of a ſolar focus as ſtrong as that from which we had obtained ſo little effect with the lenſes we had employed, I was deſirous of terminating the experiment with the great lens of Tſchirhauſen; and the claſs granted me permiſſion to take it from their cabinet.

This lens, as is well known, is 86·6 centimetres (32 inches) in diameter, and 211·076 (73 inches) focus. We augmented its power ſtill more by catching the luminous cone with the ſmall lens of the cabinet of the Inſtitute, the diſk of which is 37·89 centimetres, and the focus 56·83, which in this poſition was ſhortened to 5·41 centimetres.

A firſt ſitting gave ſcarcely any ſigns of a commencement of combuſtion. Next morning, the luminous diſk having fallen on one of the parts of the bell which was thickeſt, it occaſioned it to crack. It was therefore no longer poſſible to compare the volume of the gas before and after the operation, nor to diſtinguiſh and aſcertain the quantities of the products. We confined ourſelves to making lime-water paſs through the interior of the bell before the fiſſure had ſuffered a ſenſible quantity of common air to enter, and we obſerved that it was much troubled.

The diamond which had been laſt expoſed was noways changed at its ſurface: it had, however, loſt two decimilligrammes of its weight; which was verified by the ſame balance with which it had been weighed, and which is capable of marking, in a very ſenſible manner, theſe fractions of the milligramme. Thus we were obliged to adjourn the experiment till the next ſummer, in order to find a more favourable ſun, and to have time to provide a new apparatus.

[To be continued.]

XIII. *Account*

XIII. *Account of the Proceſſes uſed by* Mr. SHELDRAKE *to ſeparate the Mucilage from Linſeed Oil, and to diſſolve Copal in Spirit of Turpentine, and in Alcohol* *.

To ſeparate the Mucilage from Linſeed Oil.

I HAD read in ſome book, the title of which I do not recollect, that linſeed oil might be purified by ſhaking it with water, which would imbibe the impurities, and leave the oil more limpid. I tried this experiment by ſhaking linſeed oil with warm water; and was ſurpriſed to find they did not ſeparate, but remained united in the form of emulſion. I then boiled them together, and found their tendency to ſeparate diminiſh. As it is the known property of gums or mucilage to keep oil and water united in this ſtate, I was induced to ſuſpect the preſence of mucilage in linſeed oil.

I had read in Doſſie a method of preparing what he calls *fat oil*. It is effected by placing linſeed oil in a ſhallow veſſel expoſed to the heat of the ſun, and ſtirring it frequently: in a certain time it loſes its property of drying, thickens, and acquires a degree of tenacity that makes it proper for a ſize or cement for gilders, &c. A ſimilar ſubſtance is alluded to by Leonardo da Vinci.

Taking the exiſtence of mucilage in this oil for granted, I conjectured that the alteration, produced in its texture by Doſſie's proceſs, aroſe from the evaporation of ſome of its principles, and the more intimate union of the reſt in conſequence of that evaporation. To verify this conjecture, I tried the following experiment:

I filled a half-pint phial full of linſeed oil, corked and tied it ſecurely over with a bladder. This I expoſed to the heat of the ſun in ſummer, during the whole day: after it had remained a few days in this ſituation, the upper part of the

* From the *Tranſactions of the Society for the Encouragement of Arts*, &c. Vol. XVII. The preſent may be conſidered as a continuation of Mr. Sheldrake's paper on Painting in Oil, in a manner ſimilar to that practiſed in the ancient Venetian School, publiſhed in Vol. XVI.: for a copy of which ſee *Phil. Mag.* Vol. I.

phial

phial was covered with drops similar to those produced by holding a wet bottle to dry before the fire. I then shook it well, which made the contents look muddy, and set it to rest again. After a time it became clear, and a portion of transparent liquor, like water, lay at the bottom. I then repeated the shaking and setting it to rest, till no additional quantity of this fluid was separated.

By trying this experiment repeatedly upon oils procured from different places, I found that some oils afforded much more mucilage than others. From some I separated a third part of mucilage; from others, a pint would not afford more than a table-spoonful, and sometimes less. Whether this difference in the result was radically in the oils, or from a difference in the processes conducted by means so variable as the heat of the sun, I am not able to ascertain.

Upon trying the same experiment with nut and poppy oil, I found the same result, but in a different degree. The average quantity afforded by nut oil was, I believe, not more than a third part of the average of linseed oil; and the average of the poppy oil was not so much as a sixth. In some cases, particularly of the poppy oil, I did not obtain any.

The colour of the oil always diminished as the mucilage was abstracted; but the mucilage was always as colourless as water. It is a question I will not pretend to decide, whether the colour of these oils depends upon the presence of the mucilage, or upon any other principle which is destroyed by the action of light. I have in some instances had the oil as colourless as water.

This decomposition of the oils, if it may be so called, is curious, as proving the mucilage in them; but, as it is very troublesome, may it not be advisable to prefer those which have naturally the least mucilage in their composition?

After pouring the oils from the mucilage, I put several quantities of the latter together, and found them mix without any difficulty. I mixed this mucilage with water, and found it unite with it in any proportion without becoming turbid. I laid it upon plates of metal, exposing them to dry in the sun and before the fire, and, when dry, washed them with a spunge and water: but it shewed no tendency to dis-

6 solve;

folve; though, while in a liquid ftate, it feemed to poffefs all the properties of a gum.

As the proceffes by which I diffolved the amber and copal to make the oil varnifhes, are to be found in many books, and as it will be better for artifts to purchafe than attempt to make thofe varnifhes, it can fcarcely be thought neceffary to detail thofe proceffes here: but, as I believe the methods by which I diffolved the copal in fpirit of turpentine and fpirit of wine are not known, I fhall now fubjoin them.

To diffolve Copal in Spirit of Turpentine.

N. B. Whatever quantity is to be diffolved, fhould be put into a glafs veffel capable of containing at leaft four times as much, and it fhould be high in proportion to its breadth.

Reduce two ounces of copal to fmall pieces, and put them into a proper veffel. Mix a pint of fpirit of turpentine with 1-8th of fpirit of fal-ammoniac; fhake them well together; put them to the copal; cork the glafs, and tie it over with a ftring or wire, making a fmall hole through the cork. Set the glafs in a fand-heat fo regulated as to make the contents boil as quickly as poffible, but fo gently that the bubbles may be counted as they rife from the bottom. The fame heat muft be kept up exactly till the folution is complete.

It requires the moft accurate attention to fucceed in this operation. After the fpirits are mixed, they fhould be put to the copal, and the neceffary degree of heat be given as foon as poffible. It fhould likewife be kept up with the utmoft regularity. If the heat abates, or if the fpirits boil quicker than is directed, the folution will immediately ftop, and it will afterwards be in vain to proceed with the fame materials; but if properly managed, the fpirit of fal-ammoniac will be feen gradually to defcend from the mixture and attack the copal, which fwells and diffolves, except a very fmall quantity which remains undiffolved.

It is of much confequence that the veffel fhould not be opened till fome time after it has been perfectly cold. It has twice happened to me, on uncorking the veffel when it was not warm enough to affect the hand, that the whole of the contents were blown with violence againft the cieling. It is likewife

likewise important that the spirit of turpentine should be of the best quality. I have never succeeded with that which is sold at the colour-shops; but whenever I procured my spirits at Apothecaries' Hall, I have dissolved the copal, by the process I have described, without difficulty.

This varnish is of a rich deep colour when viewed in the bottle, but seems to give no colour to the pictures it is laid on: if left in the damp, it remains sacky, as it is called, a long time; but if kept in a warm room, or placed in the sun, it dries as well as any other turpentine varnish; and when dry, it appears to be as durable as any other solution of copal.

To dissolve Copal in Alcohol.

Dissolve half an ounce of camphire in a pint of alcohol; put it in a circulating glass, and add four ounces of copal in small pieces; set it in a sand-heat, so regulated that the bubbles may be counted as they rise from the bottom; and continue the same heat till the solution is completed.

Camphire acts more powerfully upon copal than any substance that I have tried. If copal is finely powdered, and a small quantity of dry camphire rubbed with it in the mortar, the whole becomes in a few minutes a tough coherent mass. The process above described will dissolve more copal than the menstruum will retain when cold. The most economical method will therefore be to set the vessel which contains the solution by for a few days; and when it is perfectly settled pour off the clear varnish, and leave the residuum for a future operation.

This is the brightest solution of copal that I have seen: it is an excellent varnish for pictures, and may perhaps be found to be an improvement in fine Japan works; as the stoves used in drying those articles may drive off the camphire entirely, and leave the copal pure and colourless on the work.

N. B. Copal will dissolve in spirit of turpentine, by the addition of camphire, with the same facility, but not in the same quantity, as in alcohol.

At the time I determined to lay the preceding papers before the Society, I conceived that the quick and certain manner

manner in which the vehicle dried, was one of its advantages. But as that circumftance has been objected to, and in fome cafes really is a difadvantage, I have fince endeavoured to remove that objection by the following procefs:

Put a pint of nut or poppy oil into a large earthen veffel; make it boil gently upon a flow fire; put in by degrees two ounces of cerufe, and ftir it continually till the whole is diffolved.

Have ready a pint of the copal oil varnifh heated in a feparate veffel; pour this by degrees into the hot oil, and ftir them together till all the fpirit of turpentine is diffipated; let it then be fet by till cold, when it will be fit for ufe.

It is obvious, that, as this is a compound of the copal varnifh with the leaft exceptionable of the drying oils, it will partake of the properties of each of its component parts. It gives lefs brightnefs and durability to colours than the varnifh will, but more than oil: but as it certainly may be ufed in painting in the fame manner as any other drying oil, and gives more brightnefs and durability to colours than they can derive from any other oil, it is not unreafonable to fuppofe that it will prove an advantageous vehicle.

I have mentioned fpecific quantities of the ingredients; but it is eafy to fee that the relative proportions may be varied according as it is required to dry fafter or flower. It muft be remarked too, that whenever the mixture is to be made, both the ingredients fhould be hot; becaufe, if either of them is cold, the mixture becomes turbid, and a part, often the whole of the copal, is precipitated: but this inconvenience is avoided by mixing and boiling them together, as I have directed. It muft likewife be obferved, that after fome time a fpontaneous alteration takes place, which diminifhes, and at laft deftroys the drying quality of this mixture: it will therefore be advifable to ufe it frefh, or at leaft not to ufe it after it has been made more than a month or fix weeks.

XIV. *Procefs*

XIV. *Proceſs for producing the Lights in Stained Drawings. By Mr.* FRANCIS NICHOLSON, *of Rippon, Yorkſhire*.*

THE difficulty of preſerving the lights in ſtained drawings, with freedom and preciſion, is ſo univerſally felt by thoſe who cultivate that branch of the arts, the practice of which is every day growing more extenſive, that the ſtatement of this circumſtance alone is ſufficient for the introduction of the following proceſs, by which that difficulty is removed, and by which all the effect of body-colour may be obtained without any of its inconveniences or defects. It is applicable to every ſubject, to the richneſs of foliage, of rocks, or of foreground; and in ruins, their moſt pictureſque appendages of hanging ſhrubs, weeds, &c. may be expreſſed by it with the utmoſt ſharpneſs, and with all the lightneſs and freedom of which body-colour or oil-painting are capable.

The principle of this proceſs conſiſts in covering the places where the touches of light are intended to be, with a compoſition not liable to be diſplaced by waſhing over it with the colour, and ſuch as may be afterwards removed by a fluid in which the colours uſed in water are not ſoluble.

This compoſition, or ſtopping mixture, is made by diſſolving bees-wax in oil of turpentine, in the proportion of one ounce of wax to five ounces of the oil; and, as near the time of uſing it as may be convenient, grind with the pallet-knife as much flake white, or white lead, in oil of turpentine, as may be wanted at one time; dilute it with the above ſolu-

* The Society for the Encouragement of Arts, &c. having laſt ſeſſion received from Mr. Nicholſon, of Rippon in Yorkſhire, a Drawing intended as a ſpecimen of the proceſs for producing the lights in ſtained drawings, by removing, after the ſhadows are waſhed in, the colour where the lights are required, giving by this means the effect of body-colour with greater clearneſs, and without any of its diſadvantages; and it appearing that Mr. Nicholſon's method of tinting drawings promiſes to be of uſe in the practice of drawing in water-colours, and produces a more ſpirited effect, the Society agreed to Mr. Nicholſon's propoſal, and purchaſed from him, at the price of twenty guineas, the complete proceſs for performing the work, as communicated in the above paper.

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tion until it will work freely with the pencil, and appear on the paper, when held between the eye and the light, to be opaque. It is neceffary to obferve this, or the firft touches will not be fufficiently vifible, after being wafhed over with the colours, to afcertain the places of the fecond. It is alfo neceffary to ufe a frame inftead of the drawing-board, or to pafte the paper on the frame of the drawing-board fo as to remove the pannel; becaufe the firft and fecond touches muft be put on with the drawing placed between the eye and the light, as they will be moft vifible in that fituation. On this frame pafte the paper wet, fo as to dry firm: when quite dry, draw the outline, and proceed as follows:—

1ft, With a fine fmall hair pencil, and the ftopping mixture, cover thofe places where the clear whitenefs of the paper may be wanted, except in the fky: let it dry a few minutes; then wet the paper on both fides, and while it is wet wafh the fky. The fhadows of the clouds, diftances, and general breadths of fhadow, muft be put in with the grey tint; and over the places of the light, wafh the tints of the brighteft light; thofe will be generally yellow ochre or light red.

The light of the clouds may be preferved fharp by preffing on that part a piece of tiffue-paper previous to the wafhing of the fky; this, by abforbing the fuperfluous moifture, will prevent the colour from fpreading farther than is defired. Suffer the whole to be very dry; and,

2dly, Touch in with the ftopping mixture, the fharp and prominent parts of the brighteft lights; let them dry a few minutes, then wafh over them with the tints of the next degree of light.

3dly, Stop with the mixture the fecond order of touches, and wafh over them with the middle tints; ftrengthen alfo at the fame time the breadths of fhadow.

4thly, Stop, with broad touches of the mixture, the places of the middle tint; uniting them to the former touches, and extending them fo as to graduate the middle colours into the fhadow: ftrengthen the fhadows, making them nearly as dark as they are intended to be, and let the whole be perfectly dry.

Then

Then take oil of turpentine, and with a ſponge, or hog's-hair pencil, waſh over the places where the mixture has been uſed, rubbing it with the bruſh until it be diſſolved: clear it away with a linen rag, and waſh it with more oil of turpentine ſo long as any white lead appears; then let it dry.

Warm the drawing; then with a ſoft bruſh and highly-rectified ſpirit of wine waſh the places where the oil of turpentine has been uſed, to clear away the remainder of it: rub the drawing lightly on the face, but ſponge it well on the back.

When dry, tint down the lights where it may be wanted; harmoniſe the colouring, and cut the ſhadows to effect, with ſtill darker tints as may be neceſſary.

If other touches of light ſhould afterwards be wanted in the ſhadowed parts, the colour may be eaſily removed by a pencil formed of ſponge, with water ſufficient to produce them with as much ſtrength as can be deſired; then ſtop them with the mixture; waſh the ſhadow over the touches, bringing it to the colour taken off; and, when dry, remove the mixture with the oil of turpentine and ſpirit of wine.

XV. *An Account of Mr.* BROWN'*s Travels through Egypt and Syria*, &c.

[Continued from page 414 of the laſt Volume.]

DURING three or four days enſuing, Mr. Brown ſuffered ſo violent a relapſe as to be unable to perform the common offices of life, and even to ſuppoſe that it was nearly at an end. The moment any ſymptoms of amendment appeared, he ſent word to the Melek that he wiſhed to be introduced to the Sultan, and then, as ſoon as poſſible, to be diſmiſſed. No reply was made to this meſſage; but the following day he came to the tent, with ſome of his attendants, and deſired to ſee the merchandiſe he had brought with him. As to part of the articles, conſiſting of wearing apparel, &c. ſuited to the great, our traveller readily conſented: but this was not ſufficient; the Melek inſiſted on ſeeing the contents of a ſmall cheſt, which chiefly contained articles uſeful to himſelf, but not

not defigned for fale. There were alfo in it fome Englifh piftols, which he intended to avail himfelf of as prefents at Sennaar, or wherever elfe he might be able to penetrate. Mr. Brown, therefore, pofitively refufed to open the cheft: the Melek threatened to have it broke open; and, as his attendants were proceeding to do fo, Ali Hamad, the man who attended Mr. Brown, took the key from its concealment and opened the box. Every thing was pulled out and examined, and many fmall articles appeared no more: the piftols were referved to be taken by the Sultan, after a violent but fruitlefs altercation at the valuation made by his own fervants; and Mr. Brown's telefcopes, books of which they knew not the ufe, with his wearing apparel, &c. were gracioufly left him. The valuation was made the following day: the whole was eftimated at thirty-eight head of flaves, being at the market price worth eighty, exclufively of a prefent of value for the Sultan. A pair of double-barrelled piftols, filver mounted, which coft in London twenty guineas, were valued at one flave; which can in general be purchafed, by thofe who are experienced in that traffic, for the value of fifteen piaftres in Egyptian commodities. On this Mr. Brown exclaimed, that if they meant to plunder, and if bargain and fale were not conducted in the country by the confent of the parties, but by force, it would be better to take the whole gratis. No anfwer was made; but the day following two camels were brought him as a prefent.

The violent manner in which our traveller's property had been feized, and the general ill-treatment he received, had much augmented his diforder. He had been fifteen days in the tent expofed to great variations of temperature; and it being at the clofe of the rainy feafon, he could rarely obtain water to drink, though tormented with thirft. He judged, therefore, that the only means of reftoration were, to return to Cobbé, and avail himfelf of the fhelter of a clay houfe and privacy, the want of which he had fo fenfibly felt. The Melek, being in poffeffion of the greater part of his property, having left him only as much as would fupply the wants of a few months, did not feem very anxious about his ftay. Mr. Brown hired therefore two Arabs, and with the camels given

to

to him, and the property that remained, arrived on the third day at the place from which he had come. In the intervals of his illnefs he vifited the chief perfons of the place; and as the eyes of the people became habituated to him, he found his fituation growing fomewhat more tolerable. Though idle during the courfe of the winter refpecting the immediate objects of his voyage, he grew, of courfe, more familiar with the manners and particular dialect of the country; for the Arabic fpoken in it differs materially from the vernacular idiom of Egypt.

The following fummer (1794) Mr. Brown, having in fome degree recovered his ftrength, determined to go and refide for a time near the Sultan, both to fupplicate for redrefs of what he had already fuffered, and to embrace any opportunity that might offer of preffing his requeft for permiffion to advance. On his arrival at El Fafher, his good friend Melek Mifellim being employed by his mafter in the fouth, he went under the protection of the Melek Ibrahim, one of the oldeft perfons in authority there, and took up his lodging in the houfe of a man named Mufa. During Mr. Brown's ftay at El Fafher, of three entire months, he was folicitous to attend regularly the levees of the Sultan, which were from fix in the morning till ten; but could very rarely obtain admittance, and when he did he had no opportunity of fpeaking. After waiting in fruitlefs expectation at El Fafher, when the time of his departure was drawing near, an accident happened, which, though not of the moft pleafing kind, contributed to make him noticed, and obtained for him at length an interview with the Sultan. One day, as he was reading in his hut, a female flave belonging to the houfe, a girl about fifteen, came to the door of it, when, from a whim of the moment, he feized the cloth that was round her waift, which dropped, and left her naked. Chance fo determined that the owner of the flave paffed at that time, and faw her. The man immediately threw his turban on the earth, and exclaimed—Ye believers in the Prophet hear me! Ye faithful avenge me! (with other fimilar expreffions:) a Caffre has violated the property of a defcendant

of Mohammed! When a number of people was collected around him, he related the fuppofed injury he had received in the ftrongeft terms, and exhorted them to take their arms and facrifice the Caffre. He had charged a carbine, and affected to come forwards to execute his threats, when fome one of the company, who had advanced fartheft and faw Mr. Brown, called out to the reft that he was armed, and prepared to refift. It was then agreed among the affembly that fome method of punifhment might be found which promifed more fecurity and profit to the complainant, and would be more formidable to the guilty. The man who acted as his broker was to take the flave as if fhe had really been violated, and agreed to pay whatever her mafter fhould charge as the price. The latter had the modefty to afk ten head of flaves; and if Mr. Brown carried the matter before the Cadi, which he fuppofed he would hardly venture to do, he had fuborned witneffes to prove that he had received of him property to that amount.

On Mr. Brown's removal from Cobbé to El Fafher, he had caufed his fmall remaining property, among which were a few articles of value, but many of much ufe to him, to be lodged in the houfe of Hoffein, the owner of the flave, and his companion. On his return thither, which happened a few days after the accident, he claimed it; but they refifted, as they alleged, at the fuit of his broker, and would not deliver it till the value of ten flaves fhould be paid to him. Mr. Brown from the firft confidered their conduct as fo violent that if it reached the ears of government the claim would unqueftionably be abandoned; and, indeed, his adverfaries had refted their expectations only on the timidity which they had been accuftomed to obferve in Chriftians of the country, whofe accufation and condemnation are in fact the fame. He had not neglected to give the tranfaction all the notoriety he could without having recourfe to public authority, and thofe to whom he had applied were decidedly in his favour. He therefore now went to his adverfaries, Hoffein and his companion, and in their prefence offered to Ali Hamad a promiffory note for the value of ten flaves at the market-

market-price on his arrival at Kahira. It was however refufed; and his cheft, containing fome German dollars and other articles, was ftill detained. The reft was given up.

Mr. Brown had been told that the Sultan was apprifed of the tranfaction previoufly to his departure from El Fafher, and that he intended to grant him redrefs; but after waiting about fifteen days without hearing any thing farther of his intentions, being weary of fuffering, he determined to return. He had been arrived but a fhort time when a *fulganaway*, or meffenger, came exprefs from Court, with orders for him to repair to El Fafher immediately. The object of the meffage was kept a profound fecret, nor could he difcover whether it portended good or evil. He left Cobbé the fame evening, and arrived at the end of his journey the day following about noon. He repaired as before to the Melek Ibrahim, who on the following day introduced him at the public audience. The Sultan, as he retired to the palace after it was over, ordered all the parties to appear. Being come within the inner court, he ftopped the white mule on which he was mounted, and began a fhort harangue, addreffing himfelf to Hoffein and Ali Hamad Mr. Brown's fervant, in which he cenfured, in a rapid and energetic ftyle, their conduct towards him:—"One," faid he, turning to Ali, "calls himfelf Wakil of the Frank; if he were a Sherif and a Muflim, as he pretends, he would know that the law of the Prophet permits not a Muflim to be Wakil to a Caffre: another calls himfelf his friend: both are agreed in robbing him of his property, and ufurping the authority of the laws. Henceforth I am his Wakil, and will protect him." He then ordered the parties to repair to the houfe of Mufa Wullad Jelfûn, Melek of the Jelabs, under whofe appropriate jurifdiction are all foreign merchants. Mr. Brown here gives the following account of the manner in which he had been before received by the Sultan:

"On my firft audience," fays Mr. Brown, "I was too ill to make much obfervation. He was feated at a diftance from me; the vifit was fhort; and I had no opportunity of opening a converfation. He was placed on his feat (*cûrfi*) at the door of his tent. Some perfon had mentioned to him

my watch and a copy of Erpenius's grammar I had with me. He aſked to ſee both; but after caſting his eyes on each, he returned them. The preſent I had brought was ſhewn him; for which he thanked me, and roſe to retire.

"During the following ſummer the firſt time I got admiſſion to him he was holding a divan in the outer court. He was then mounted on a white mule with a ſcarlet *beriſh*, and had on his head a white turban; which however, together with part of his face, was covered with a thick muſlin. On his feet were yellow boots; and the ſaddle on which he was ſeated was of crimſon velvet, without any ornament of gold or ſilver. His ſword, which was broad and ſtraight, and adorned with a hilt of maſſy gold, was held horizontally in his right hand. A ſmall canopy of muſlin was ſupported over his head. Amid the noiſe and hurry of above a thouſand perſons, who were there aſſembled, I was unable to make myſelf heard, which the nature of my ſituation enabled me to attempt, though not exactly conformable to the etiquette of the court, that, almoſt to the excluſion of ſtrangers, had appropriated the divan to the troops, the Arabs, and others connected with the government.

"On another occaſion I contrived to gain admittance to the interior court by a bribe. The Sultan was hearing a cauſe of a private nature, the proceedings on which were only in the Furian language. He was ſeated on a kind of chair, which was covered with a Turkey carpet, and wore a red ſilk turban; his face was then uncovered; the imperial ſword was placed acroſs his knees; and his hands were engaged with a chaplet of red coral. Being near him, I fixed my eyes on him in order to have a perfect idea of his countenance; which, being ſhort-ſighted, I had hitherto ſcarcely found an opportunity of acquiring. He ſeemed evidently diſcompoſed at my having obſerved him thus, and the moment the cauſe was at an end he retired very abruptly. Some perſons to whom I afterwards remarked the circumſtance, ſeemed to think that his attendants had taught him to fear the magic of the Franks, to the operation of which the habit of taking likeneſſes is imagined by ſome of the Orientals to conduce. He is a man rather under the middle ſize, of a

complexion

complexion aduſt or dry, with eyes full of fire, and features abounding in expreſſion: his beard is ſhort, but full; and his countenance, though perfectly black, materially differing from the negro: though fifty-five years of age, he poſſeſſes much alertneſs and activity.

"At another of my viſits I found him in the interior court, ſtanding with a long ſtaff tipped with ſilver in his right hand, on which he leaned, and the ſword in his left. He then had choſen to adorn his head with the folds of a red ſilk turban, compoſed of the ſame material as the weſtern Arabs uſe for cincture. The Melek Ibrahim preſented him, in my name, with a ſmall piece of ſilk and cotton of the manufacture of Damaſcus. He returned in anſwer, *Barak ulla fi!* May the bleſſing of God be on him! a phraſe in general uſe on receiving any favour; and inſtantly retired, without giving me time to urge the requeſt, of which I intended the offering ſhould be the precurſor. It is expected of all perſons, that, on coming to El Faſher, they ſhould bring with them a preſent of greater or leſs value according to the nature of the buſineſs in hand. It is no leſs uſual, before leaving the royal reſidence, to aſk permiſſion of the Sultan for that purpoſe. With this latter form, which was to me unpleaſant, I ſometimes complied, but more frequently omitted it. But on this occaſion, having been long reſident there, I thought fit to make a laſt effort to promote my deſign. The day preceding that which I had fixed for my return, happened to be a great public audience. I found the monarch ſeated on his throne (*cürſi*) under a lofty canopy, compoſed not of one material, but of various ſtuffs of Syrian and even Indian fabric, hung looſely on a light frame of wood, no two pieces of the ſame pattern. The place he ſat in was ſpread with ſmall Turkey carpets; the Meleks were ſeated at ſome diſtance on the right and left; and behind them a line of guards, with caps ornamented in front with a ſmall piece of copper and a black oſtrich feather. Each bore a ſpear in his hand, and a target of the hide of the hippopotamus on the oppoſite arm: their dreſs conſiſted only of a cotton ſhirt of the manufacture of the country. Behind the throne were fourteen or fifteen eunuchs, clothed indeed ſplendidly in habiliments of cloth

or ſilk, but clumſily adjuſted, without any regard to ſize or colour. The ſpace in front was filled with ſuitors and ſpectators to the number of more than fifteen hundred. A kind of hired encomiaſt ſtood on the monarch's left hand, crying out a *plein gorge* during the whole ceremony:—"See the buffalo, the offspring of the buffalo; a bull of bulls; the elephant of ſuperior ſtrength; the powerful Sultan Abd-el-rachmán-el-raſhîd! May God prolong thy life, O maſter! May God aſſiſt thee, and render thee victorious!" From this audience, and thoſe which preceded it, I was obliged to retire as I had come, without effecting any thing."

Mr. Brown's reception with Muſa Wullad Jelfûn was very different from that which he had experienced in the houſe of Miſellim or Ibrahim. His behaviour towards him was complacent, and he affected to ſeek opportunities of hearing his ſentiments on ſuch ſubjects as occurred. In obedience to the Sultan's command, Mr. Brown now gave in an exact ſtatement of the property he had loſt, and ſubſtantiated the proof by the ſtrongeſt circumſtantial evidence. With regard to the ſlave, the moſt complete redreſs was afforded him: the charge brought againſt him was judged abſolutely futile, and ſhe was reſtored to her maſter; while he, on the other hand, was compelled to give up the cheſt, &c. which had been violently with-held. The plunder which had fallen into the hands of his ſervant, and his accomplice, was not ſo eaſily reſtored. The Melek, tired of a gratuitous juſtice, began to think that a lucrative compoſition was more eligible. The offenders, who had been obſtinate in the firſt inſtance, ſeeing how the cauſe relative to the *jarea* (female ſlave) had been decided, thought proper to offer to the Melek marks of their gratitude for the lenity they expected from him; and the Sultan was unwilling to imagine that the ſufferings of a Caffre could fall heavy on himſelf at the day of final retribution. At length the Melek, who in reality was ſupreme arbiter of the conteſt, contented himſelf with giving Mr. Brown the intrinſic value, about four head of ſlaves, inſtead of twenty-four or twenty-five, which at firſt he had unequivocally declared due to him, and promiſed he ſhould receive. Thus the matter was terminated.

[To be concluded in the next Number.]

XVI. *De-*

XVI. *Description of the Paliorum Lacus, or Lake Palius, in the Valley of Noto in Sicily.* *By* M. DOLOMIEU *.

THIS ſmall but very remarkable lake, ſituated at the diſtance of two miles weſt from the town of Palagonia, or of a mile from Mineo, is ſurrounded by volcanic mountains, and lies in a ſmall plain ſomewhat hollow towards the middle. This plain is half ſurrounded by ſteep rocks, which give it the appearance of a monſtrous crater, that has ſunk down by ſome convulſion. The lake is placed in its centre, and, as it were, in the middle of a funnel. Its depth often varies, and conſequently its circumference. In the winter time it may be about ſixty or ſeventy fathoms in diameter, and about ten in depth; but in ſummer, when great drought prevails, it is often entirely dry. At the period when I ſaw it, which was in the month of May, it formed an oval about thirty fathoms in length and twenty in breadth: its depth was about five or ſix. It had a ſtrong ſmell of Jew's pitch, or aſphaltes, even at a conſiderable diſtance. The water in its colour inclined a little to green, and had an exceedingly nauſeous and diſagreeable taſte. I was told that the water was often tepid; but it had, at the time when I examined it, the temperature of the atmoſphere. In ſeveral parts of it I obſerved a violent bubbling, and particularly in four places near the middle. This bubbling was ſtronger at certain intervals, and the water was thrown up ſometimes to the height of two or three feet, riſing in this manner every five or ſix minutes. There are periods when this boiling is ſtronger or weaker; but on theſe occaſions no other noiſe is heard than that produced by the motion of the water.

When the lake is dry, one may without danger examine its centre, where there are ſeveral deep holes. From theſe holes currents of air, ſomewhat warm, continually ariſe, and throw back ſand and other bodies if put into them. It is this aëriform vapour which, when the baſon is full, forces up the water as before deſcribed, and makes it to be covered

* From *Magazin für das neueſte aus der Phyſik*, Vol. III.

with

with foam. It would, no doubt, be important to examine accurately the nature of this vapour; but though I was provided with the proper apparatus, I could not obtain my object, becaufe I durft not venture to wade into the water in order to reach the places where the bubbling appeared, and which were at a confiderable diftance from the bank.

The mud at the bottom and on the bank, which has a black colour, is tenacious, and fmells like pitch. A little petroleum has alfo been fometimes found at the furface of the water. The whole foil of the fmall plain confifts of black, tough, refinous, inflammable earth. A few years ago fome ftraw huts in this neighbourhood having been fet on fire, the fire was communicated to the ground, which burnt with a whitifh dull flame, like that of the inflammable fprings in Dauphiny, during feveral months, and was extinguifhed with the greateft difficulty, as the fire, when deftroyed in one place, broke out in another. Since that period great care is taken not to kindle fire in the neighbourhood. From this phenomenon I am induced to think, that the air which rifes through the water of the lake, and which probably finds a paffage through the ground, may be of an inflammable nature alfo; as the air of marfhes, which burns without any noife. The fertility of this fmall plain is fo great, that it produces a moft abundant crop every year, without requiring much agricultural care. In walking over the ground a hollow noife is heard; which feems a proof of there being beneath the furface fubterranean cavities, like thofe of Solfatara near Puzzuolo. From thefe circumftances there is reafon to think that this place has been formed from the remains of a fallen crater, a part of which is ftill feen in the furrounding mountains; and between this lake and that of Agnano near Naples, there is no other difference than the greater quantity of water in the one, and the more violent efflux of vapour in the other. Some affert that the vapour of this lake is mortal; and that no bird, or other animal, can be expofed to it without being killed. The vapour alfo which arifes from the ground is confidered as of a fuffocating nature, fo that people who lie down on the earth, or only bend their bodies towards it, are expofed to the greateft danger; though one

may walk over it without the leaſt fear of any pernicious conſequences. All theſe phenomena have a great ſimilarity to thoſe of the Grotto del Cane, near the lake Agnano.

On the banks of the lake Palius there are found a great many ſmall cluſters of aſhes and ſlag, exactly like thoſe adhering to the ſides of the craters of Ætna and Veſuvius. The mountains and lava around this lake exhibit the moſt evident marks of their great antiquity, as in many places they are covered with calcareous ſtones, from which it may with certainty be inferred that they muſt have originated at a time when the preſent continent was not inhabited. In the time of Diodorus Siculus, however, the crater, which forms the preſent baſon of the lake, ſhewed traces of its internal inflammation: as we are informed by this hiſtorian, that flames burſt forth from this ſpot; that the water poſſeſſed a conſiderable degree of warmth; and that a horrid thundering noiſe was heard. Being extremely deſirous to know whether a connection actually exiſted between this volcano and that of Ætna, as they are at no great diſtance from each other, I enquired whether any affinity had ever been obſerved between the eruptions of Ætna and the before-deſcribed phenomena of the lake; and whether, during theſe eruptions, a ſtronger ebullition had taken place: but I was aſſured that no perſon had ever obſerved any thing of the kind.

The phenomena of this lake have at all times been attended with ſo many ſingularities as to give riſe to fables without number: at preſent it is ſaid to be inhabited by a fairy. In ancient times theſe phenomena were aſcribed to the ſupernatural power and influence of ſome deity. On this account a very celebrated temple, the remains of which I could not find, notwithſtanding all the trouble I took to diſcover them, was erected here to Jupiter and the nymph Thalia. People ſwore alſo by this lake, with the ſame reſpect and ſolemnity as by the river Styx. What aſtoniſhed the ancients, and what even at preſent excites the wonder of all thoſe who viſit this lake, is the inceſſant ebullition of the water, though the leaſt increaſe is not obſerved in it on the bank.

In the neighbouring mountains I found under the lava a

reſinous

refinous ſtrong-ſmelling ſubſtance in horizontal ſtrata, which could be eaſily divided into laminæ of from one to two inches in thickneſs; but, in other reſpects, the lava of this volcano exhibits nothing but what has been ſeen in the other extinguiſhed volcanoes in the neighbourhood. The Val di Noto is the only part of Sicily where traces of ancient volcanoes are found. Travellers, who imagined that they found ſome of the like kind in other provinces, were either miſtaken, or ſuffered themſelves to be deceived by falſe marks.

XVII. *Obſervations on the Nature of the Fog of* 1783. *By* M. De Lamanon, *Correſpondent of the Academy of Sciences at Paris**.

When this fog, which may be called an electric fog, began to appear, I was at Sallon de Crau, in Provence. In order to free my neighbours and countrymen from uneaſineſs as much as poſſible, I wrote a letter to M. Artaud, editor of the *Courier d'Avignon*, in which, after ſpeaking of the nature of the fog, I ſaid it would be deſtroyed by the ſtorms that would not fail to enſue. The event fully juſtified this kind of prediction. Having learned from the public papers that this phenomenon was not local, but almoſt general throughout Europe, I made new obſervations, and traverſed the higheſt Alps of Provence, Dauphiny, and Piedmont; and, during the courſe of my travels, collected information reſpecting the fog, and the effects of the thunder. But before I offer that explanation of the phenomenon, which appears to me moſt probable, let me endeavour to give an accurate deſcription of it.

I. *Nature and Effects of the Electric Fog.*

1. In almoſt all countries the fog was preceded by a ſtorm.

2. It began the ſame day at places very remote from each other; as Paris, Sallon, Turin, Padua, &c. where it appeared, for the firſt time, on the 18th of June. M. Senebier

* From the *Journal de Phyſique.*

wrote

wrote to the Count de Saluces, perpetual prefident of the Academy of Turin, that the fog was obferved at Geneva on the 17th. I read in the *Affiches* of Dauphiny, that it began at Grenoble on the 21ft.

3. A north wind prevailed in feveral places when the fog began; and in other places, where it began the fame day, a fouth wind prevailed. The fog, after having ceafed, re-appeared in fome places by the north wind; in others, by an eaft, weft, or fouth wind.

4. The atmofphere was not every where equally dry. At Sallon I obferved that it did not make falts enter into deliquefcence; did not raife the hygrometer; did not prevent evaporation from being abundant; and did not even tarnifh glafs, which I expofed to it. The falt-pits at Hyeres, in Provence, cryftallifed a fortnight fooner than ufual by the effect of the fog. Meffrs. Toaldo and Senebier obferved, the one at Padua and the other at Geneva, that the hygrometer did not reach the point which denotes humidity. In the Champfaur of Dauphiny, and at Turin, the fog was fometimes humid.

5. The fun, which was never feen but through the fog, appeared very pale in the day-time; of a blood-red colour at rifing, and ftill more fo at fetting.

6. At Sallon the fog fometimes diffufed a very difagreeable fmell, difficult to be determined, and which fome believed to be fulphureous. This bad fmell was perceived in other places.

7. It was hurtful to the eyes. At Sallon, perfons whofe lungs were weak, found difagreeable effects from it. The inhabitants of the Champfaur informed me that feveral people in that neighbourhood had violent pains in the head; and that, in general, they partly loft their appetite. The inhabitants of this valley are the greateft eaters in France: ftrangers, who refide among them, and drink their waters, eat almoft as much as they, and are not fo nice as ufual in regard to the quality of their food. This I experienced in 1783.

8. In Lower Provence, Languedoc, and other places, the fog ripened the corn, and was favourable to the harveft. The

 peafants

peasants beheld, with the greatest satisfaction, the effect of the fog on their crops; and yet were afraid of it. In Upper Dauphiny, and at Turin, it blighted several fields of wheat, and rendered copper buttons green. In other places it dried the plants.

9. At Padua, Turin, Paris, Sallon, Grenoble, the barometer remained almost always stationary at the point which denotes its mean state.

10. There were some days remarkably warm; but, in general, the months of June and July were almost every where less warm than usual. That year there was no summer on the high mountains of Provence and Dauphiny, where the shepherds of the plains of Crau and Camargue tend annually numerous flocks.

11. There were every where storms of rain; and after these storms the fog sometimes increased, but most frequently was diminished.

12. During the whole time of the fog, an electric machine I had at Sallon emitted few or no sparks. A philosophic friend at Sorgues, near Avignon, wrote to me, that his electrometer always indicated a great deal of electricity in the atmosphere.

13. On the 4th of July, at five in the morning, M. Nicolas, physician at Grenoble, and M. Plana, apothecary, took four measures of fog and mixed them with two measures of nitrous air: the absorption was 1-4th, and nothing remained but a gas, in which a candle became extinguished several times. Atmospheric air generally contains nearly 1-3d of pure air (oxygen gas), and 2-3ds of mephitic air (azotic gas). Fontana's eudiometer gave the same result on the 7th of July: of three hundred parts of atmospheric air, thirty-two were absorbed. The air of the fog, mixed with inflammable (hydrogen) gas, did not prevent it from exploding when a lighted taper was presented to the neck of the bottle in which it was contained.

14. Almost all those who have spoken of this fog, say that it was low. When I was on the top of Mount Ventoux, however, nearly 1040 toises above the level of the sea, I saw it far above me. M. Senebier says, in his letter to Count

de Saluces, that it was ſeen in the Alps at an elevation greater than that of Mount Salève, which riſes 601 toiſes above the ſea. On the 22d of September (1783) I aſcended the higheſt Alps of Dauphiny, to the height of 1660 toiſes above the ſea. (No one has yet been higher in Europe.) The ſhepherds, who ſerved me as guides, all aſſured me that this fog had however paſſed over theſe mountains.

15. The loweſt part of the fog was the thickeſt and drieſt. I aſſured myſelf of this by proceeding from the borders of the ſea to the ſummits of the higheſt mountains.

16. It is probable, according to every account, that this fog overſpread almoſt all Europe, the iſlands of the Mediterranean, and a part of Africa. It covered the whole Adriatic Sea, but extended only to the diſtance of 100 leagues on the ocean. It was properly a continental fog.

17. The thunder this year (1783) occaſioned great devaſtation. In Provence and Dauphiny alone it killed nearly ſixty perſons, and a great number of animals. I have found no inſtance of its falling and doing miſchief in places higher than 450 toiſes above the level of the ſea. It would be too tedious to inſert here the name of every place, which I noted down in my journal, where I learned that the thunder fell. It will be ſufficient to relate the moſt ſingular effects it produced, and ſuch as may add to our knowledge reſpecting the nature of theſe terrible meteors.

According to Pliny, Plutarch, Seneca, &c. the olive, fig-tree, and laurel, are never ſtruck by lightning. Porta adopted this opinion, which was that of all the ancients. Toaldo ſays alſo, it is only thoſe trees which contain reſin that can perhaps eſcape the danger of thunder; ſuch as the olive-tree, laurel, fir, and ſome others of the like kind. This, in all probability, is the foundation of the practice, common among the populace, of keeping in their houſes, and placing on the ſummits of ſteeples and at the corners of fields, branches of olive that have been bleſſed, and of burning them in their houſes during ſtorms. It is poſſible, and even probable, that trees of this kind may be leſs frequently ſtruck by lightning than thoſe which contain a great deal of aqueous juices; but I can aſſert, that theſe preſervatives of the ancients are not

equal to our conductors. On the 21ſt of June the lightning burnt the leaves of a fig-tree, and all the bark of an olive-tree, in the territory of Sallon; and I learned that the ſame thing had happened formerly, and that there even had been fig-trees and olive-trees ſplit and tore to pieces by thunder.

There is ſome kind of thunder, ſays Seneca, accompanied with a loud report, by which men fall dead, and ſome become ſtunned and loſe their ſenſes. In 1783 I ſaw ſeveral inſtances which confirm the truth of this obſervation. At Pelliſanne in particular, which is ſcarcely a league from Sallon, the thunder, attracted by an iron croſs, killed two perſons, and deprived ſeveral others, as it were, of their ſenſes. The ſame thing happened at Freiſſinouſe, in Dauphiny.

Thunder ſometimes, ſays Seneca, renders wine frozen and congealed. Of the two perſons killed by the thunder at Pelliſanne, one loſt immediately all his natural heat, and the body was found extremely cold. Thunder muſt ſometimes produce theſe effects by occaſioning a ſtrong and ſpeedy evaporation.

Bodies ſtruck by thunder, ſays Plutarch, do not corrupt: dogs and birds do not eat them. This may ſometimes be the caſe, but I know ſeveral examples of the contrary *. On the day of the fair of Villefranche (July 22d) the thunder killed a mule, which corrupted ſo ſoon that it was found neceſſary to remove it. About eight or nine years ago, ſeveral ſheep were killed by lightning on the mountain of Sederon in Provence; and the ſhepherds abſtained, for a long time, from approaching the place, on account of the bad ſmell which they emitted. On the 1ſt of July, about four in the afternoon, the lightning fell at Carpentras on the Lazaret, (a place where thoſe who died of the plague were formerly buried,) and ſet fire to the wood-work. The fire being communicated to ſeveral quintals of gunpowder, an exploſion took place, by which five perſons were killed and fifteen wounded. The ſame flaſh of lightning traverſed the convent of the Carmelites, and melted part of the ſciſſars of the prior: it killed alſo a cow, which was eaten without any bad conſequence.

* See *Mr. Achard's Experiments, in Philoſophical Magazine*, Vol. III. p. 51.

Towards

Towards the end of the month of June, the curé of Eſpinouſe, in Provence, ſtanding at the door of the church in order to exorciſe the thunder, was killed, as well as his maid-ſervant and his clerk. At Mane, in Provence, the bell-ringer was killed by it. On the 26th of Auguſt the lightning was attracted by the iron croſs on the church of Sigoger du Hai, in Dauphiny; and, going round it both on the inſide and outſide, frightened the ringer ſo much that he ſwore he would never return thither again in the time of a ſtorm. At Pernes, in the Comtat, it overturned a croſs. This croſs, the remains of which I ſaw, was of white calcareous ſtone, and faſtened by a bar of iron to a column of *coquilliere* ſtone * of a yellowiſh colour. The iron had diſappeared; a part of the column was ſhivered, and blocks of it carried to the diſtance of more than twenty-five paces.

At Aix the thunder dried up a beam, and left in one place only the fibrous part. At Freſſinouſe, in Dauphiny, it killed two oxen; deprived a labourer of his ſenſes, and carried away one of his toe-nails. I muſt here remark, that his ſhoes were ſhod with iron. At La Motte du Caiſe, in Provence, it entered at a window, where there was no iron, and went out at the chimney. At Claret it unſaddled an aſs without doing him any hurt, and carried the packſaddle to a conſiderable diſtance. In ſeveral places, and particularly at Banon in Provence, the lightning tore off the hair from the heads of ſeveral women. At Saint Criſtol, in Provence, it carried away the half of a girl's body. At Avignon it carried away the half of a cat. I had this fact from M. Sauvan, who on that day (June 21ſt) obſerved the ball on the ſteeple of the Grands Auguſtins of Avignon covered with a crown of light, which continued three quarters of an hour, and diſappeared at eleven at night.

On the 22d of July there fell a great quantity of hail at Saint Eſprit: it was of a very large ſize. A girl, ſtruck on the head by a grain of it, applied her hand to the place and found her head-dreſs on fire.

On the 21ſt of June aſcending thunder was ſeen at Sallon:

* A kind of ſtone mixed with abundance of ſhells.

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like phenomena were observed the same day in several other places. In several villages through which I passed, I was shewn a great number of trees stripped of their bark by the thunder; and I remarked that the part of the bark, or wood, carried away, was almost always broad at the bottom of the tree, and narrow at the summit. It would seem as if the lightning had met with obstacles, and that it had not the same force when it attacked the higher parts of the tree. In my opinion the thunder which produced these effects was all ascending; and I often observed holes at the roots of the trees which had been deprived of their bark. I observed also that dry land had been less struck with the lightning than moist land, which induces me to believe that the greater part of the thunder that year was ascending.

II. *Thoughts on the Origin of the Electric Fog.*

Several philosophers adopted the opinion of the populace, and considered this fog as a natural effect of the earthquake which laid waste Sicily and a part of Calabria. Toaldo thought that all these exhalations were brought from Calabria and Sicily by the winds which blew from the southward: but this respectable philosopher, at the time when he wrote, did not know that the fog was almost general throughout Europe. Besides, the earthquakes in Calabria and Sicily took place chiefly in February, and the fog did not appear till the middle of June; that is to say, till more than four months after. In my opinion, then, this fog was not occasioned by the earthquakes of Calabria and Sicily; but the fog and these earthquakes, as well as those which we are assured took place in Iceland, had a common cause, which produced different effects according as they were modified by circumstances and the nature of the places. Let us now search for this cause; we shall find it in the annals of meteorology, and nothing is necessary but to distinguish it.

I have said, in another place *, that the constitution of the atmosphere depends chiefly on the nature and form of the ground, and that the revolutions of the air are subject to the

* *Journal de Physique*, Mars 1782, p. 187.

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revolutions of the earth, over which they have an influence in their turn. If we consider the different bodies or substances of which that part of the earth known to us is composed, we shall see that, notwithstanding their apparent rest, they all obey, as we may say, an intestinal motion; and that this motion gives continual rise to new compositions and combinations, the connection of which escapes us, but which nevertheless exist. The remains of animals and vegetables still distinguishable, and which occupy so much room in our globe; the acids which attack them; the aëriform fluids disengaged from them; the metals and pyrites brought to perfection, or decomposed; the fires separated, or collected; the fermentations and effervescences; in a word, the innumerable decompositions and recompositions of all the parts of the earth, furnish abundance of subtle matter, which, by its levity, disengages itself at the surface of the globe, cannot, in certain circumstances, resist the gravity of the air, and rises sometimes to the summit of the atmosphere. On the other hand, the rains which fall, moist fogs, and several other causes, make a part of the water, which detaches itself from the air, to penetrate the earth, and to combine with the substance of these exhalations. It is afterwards, in part, attracted by the external heat, or repelled by the heat of the earth itself; but it never returns to the atmosphere as pure as it was when it issued from it. It carries with it a great part of that subtle matter of which I have spoken; and it is this afterwards which produces thunder, and almost all fiery meteors. There is then a continual communication from the earth to the atmosphere, and from the atmosphere to the earth. The greater part of these exhalations, in general, is specifically heavier than the atmospheric air; and, if they are not impregnated with a certain quantity of water, they cannot quit the earth. They must necessarily be there collected, and accumulate to a greater or less depth in the time of great droughts.

Let us now consult our registers, and those of different observers, and we shall see that there had prevailed, at least for nine years, an extreme drought, not only in Europe, but also in Africa and America. This was announced year after

year by our domeftic as well as foreign gazettes; and we may conclude fo from the minute obfervations of the celebrated Van Swinden, and the tables publifhed by Toaldo, Cotte, Beraud, Beguelin de Romily, &c. Sometimes, indeed, rain took place in one country or other; but in general, till the preceding winter, an extraordinary drought prevailed. It began about 1774, and in the month of June 1782 was extremely great in Italy and in our fouthern provinces. We experienced at that period a fuffocating heat; the earth, as we may fay, feemed to be on fire, and, in the Plain of Camargue, fcorched the feet of the reapers to fuch a degree that they were obliged at length to walk upon ftraw: feveral died of heat with the fickles in their hands, and there were a great many fick. In a word, the drought and heat were fo exceffive, that, at two leagues from Sallon, the fpiders, which in general are not venomous, occafioned by their bite violent difeafes, which had a great affinity to thofe occafioned by the bite of the tarantula.

In confequence of this great drought, the exhalations of the earth, fpecifically heavier than the air, and deprived of that humidity which ferves them as a vehicle, remained in the bofom of the earth, where they muft have formed immenfe accumulations. The winter of 1782—1783 was rainy, particularly in Calabria and Sicily; and the Alps were covered with a great deal of fnow. The fpring alfo was in general rainy. The water then being filtered into the bowels of the earth, was at firft abforbed by the very dry exhalations there confined. This humidity, added to the warmth of the fpring, no doubt occafioned effervefcences and fermentations; fo that the exhalations, difengaging themfelves with violence, in certain places convulfed the earth, as was the cafe in Calabria and Sicily. In proportion as the water filtered into the earth by its own weight, it found new exhalations, which, by difengaging themfelves, occafioned new convulfions, but lefs confiderable on account of the lefs abundance of thefe exhalations. In places where they were heated by their mixture, they liquefied ftones, and threw up volcanic iflands, as in Iceland. In the laft place, thefe fubtle exhalations rifing into the atmofphere from all parts, with the vapours, which

which ſerved them as a vehicle, did not at firſt alter its purity, being intimately connected with it; but they nevertheleſs exiſted, and produced in its higheſt regions the multiplied halos, paraſelena and parhelia obſerved that year. The heat increaſing, and the earth continuing to furniſh exhalations in proportion to the preceding humidity, theſe were communicated to the atmoſphere in a manner almoſt inſenſible: but the atmoſphere being at length ſaturated, theſe exhalations underwent new decompoſitions; ſtorms were formed, the atmoſphere was cooled, and ſuffered to eſcape a part of theſe exhalations, which fell again towards the earth, and in one day Europe was covered with a dry fog *. Local circumſtances, in regard to moiſture, winds, and clouds, exempted certain places from it for ſome time. The earth, however, continuing to furniſh exhalations, and theſe being united to thoſe which the atmoſphere, as we may ſay, had depoſited, deſcending and aſcending thunder were ſeen till the exhalations of the earth and the atmoſphere were conſumed. The atmoſphere being gradually purified, and the ſource of the exhalations exhauſted, the earth ceaſed to be convulſed.

XVIII. *Report on the Converſion of Soft Iron into Caſt Steel by means of the Diamond. Read in the French National Inſtitute, Thermidor 26, Year 7. By* C. Guyton †.

THE Claſs will recollect the account which I gave of the grand experiment of the combuſtion of the diamond in oxygen gas in the focus of the lens of Tſchirnhauſen, and the new facts which I thence deduced reſpecting the true nature of the diamond; plumbago, which is its oxyd in the firſt degree; carbon, which is its oxyd in the ſecond degree; and the carbonic acid, which is the produce of its complete oxy-

* The ſame fog, I preſume, took place in America, where there had been great complaint of drought for eight years. It was not ſeen in the open ſea, becauſe it was abſorbed by the water: for this reaſon it did not appear in countries where the ſky was overſpread with clouds.

† From the *Annales de Chemie*, No. 92.

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genation. They suggested to our brother, C. Clouet, the idea of searching for a confirmation of a new kind, by trying to make soft iron pass to the state of steel by cementation with the diamond.

It has hitherto been considered as certain, that iron does not melt but by passing to the state of steel or cast iron. But in what state does the carbon enter into that combination? It might be conjectured, that it is in the state of plumbago, or oxyd of the first degree; since that which is separated by acids exhibits the brilliant blackness and incombustibility which form its principal characters. Hence some were inclined to conclude, that the carbon entered into this union in the state of an oxidule; that consequently the carbon employed in the cementation of steel began by deoxydating itself to a certain degree. This was even in some measure proved; as the carbon employed for this operation was indeed found to have a more brilliant aspect, and nearly resisted incineration, like carbon in a mass burnt in close vessels. But if carbon really burns in the cementation of iron, it ought to disengage from it oxygen gas. This is a question which I have endeavoured to resolve by experiment.

I cemented small bits of iron in a porcelain retort, which in the preceding operation had received a vitreous coating, and which consequently was no longer permeable to air. These fragments were all surrounded, on every side, by charcoal of beech pulverised, and very dry. The retort was put into the reverberating furnace, and a tube connected to it and carried under a receiver filled with mercury. There was disengaged a quantity of elastic fluid, composed of carbonated hydrogenous gas and carbonic acid gas, the last of which was at first only 0·11 in bulk; towards the middle of the experiment, 0·13; and at the end, 0·15.

The conversion of iron into steel being found only little advanced, after three hours and a half exposure to the fire, we put the same iron and the same carbon again into the retort, and exposed it to the heat of a three-blast-furnace. This time there was only a very small quantity of gas; but it was still carbonated hydrogenous gas mixed with carbonic acid gas, and always with the same progression of the latter;

which

which made at first only 0·07 of volume, while the last portions contained 0·12. The iron on this occasion was converted into steel, and even the fragments had united by a commencement of fusion.

It was very probable that a part of the carbonic acid, collected in this operation, might have been formed at the expence of the remaining carbon and with disengaged oxygen; but the constant presence of the hydrogen only served to indicate the difficulty of freeing the carbon entirely from the last portion of water it contained. I shall here take occasion to observe, that this experiment seems not at all reconcileable with the opinion of some chemists, that hydrogen has more affinity than carbon for oxygen: an opinion which they found on this circumstance, that carbon is precipitated, in Volta's eudiometer, when a mixture of oxygen gas and carbonated hydrogen gas is made to detonate, if a quantity of oxygen sufficient to acidify the two bases has not been employed. I say, that this affinity was not exerted in my experiment: for it cannot be doubted that the temperature was high enough to reproduce water by the union of the oxygen and hydrogen; and we can here see nothing which could decide a preference of the oxygen for the carbon.

These considerations seemed to me sufficient to create a new interest in regard to the experiment proposed by C. Clouet. I did not hesitate, therefore, to employ in it one of the diamonds preserved in the cabinet of the Polytechnic school, according to the leave granted by the Council; being persuaded that if it disappeared in the operation, merely by exposure to a high temperature, in contact with iron, without the accession of the air or any other oxygenating substance, the fact thereby established would leave no room to regret having sacrificed it.

Citizen Clouet had himself prepared a small crucible of soft iron, forged on purpose out of picked heads of nails. Its form was a solid of eight planes. (Plate II. fig. 3). It was shut by a stopper of the same iron well adjusted. (Fig. 4.)

This crucible was to be placed in a Hessian crucible, furnished with a cover well luted. This was all the apparatus for the experiment. I cannot give a better idea of the

refult than by the report drawn up by C. Clouet, Welter, and Hachette.

Report of the Experiment made at the Polytechnic School, Thermidor 25, Year 7, refpecting the Converfion of Iron into Steel by the Diamond.

"The diamond employed weighed 907 milligrammes. As it did not entirely occupy the crucible, we filled it with filings of the fame iron as that of which it was formed. The crucible was fhut by its iron ftopper, which was forcibly thruft home, that as little air as poffible might remain in the infide.

		Grammes.
"The crucible and ftopper weighed together	-	55.8
"The iron filings which covered the diamond	-	2
"Total weight of the iron furrounding the diamond		57.8

"After having cut off the excefs of the ftopper *, the crucible was placed alone, and without the addition of any furrounding matter, in a very fmall Heffian crucible, and the latter in a fecond crucible of the fame earth; but the space between the two latter crucibles was filled with filiceous fand free from all ferruginous particles. In the laft place, the large crucible was luted with earth arifing from pounded crucibles and unbaked clay, and the whole was expofed about an hour to a three-blaft-forge fire.

"The whole being cooled, we found, in the interior Heffian crucible, the iron converted into an ingot of caft fteel. (See fig. 5.) It formed, with the ftopper and filings, but one round mafs well terminated, fome few globules excepted, which were detached, and which weighed only 884 milligrammes.

		Grammes.
"The ingot of caft fteel weighed	- -	55.500
"The detached globules	- - - -	0.884
"Total weight of the fteel obtained	- -	56.384

* This portion of the ftopper, as well as the remainder of the ingot of which the crucible was formed, were fubjected to the Clafs for infpection, in order to afcertain the nature of the iron employed.

"The

"The iron and the diamond weighed, before the operation, 58.707 grammes; from which it follows that there was a loſs of iron about 2.423 grammes. This iron had given to the Heſſian crucible the colour of plumbago.

"(Signed) CLOUET, WELTER, HACHETTE."

The fuſion of the iron being ſo far perfect as to ſhew on its ſurface the rudiments of the moſt beautiful cryſtalliſation, it is not poſſible to think that any part of the diamond could have remained in the inſide untouched, or that it was not in the moſt intimate ſtate of combination. The difference of the ſpecific gravity oppoſes ſuch an idea*.

Thus the diamond diſappeared by the affinity which iron exerciſed on it by the help of the high temperature to which they were both expoſed, in the ſame manner as a metal diſappears in the allay of another metal.

The diamond, therefore, has furniſhed here the ſame principle as carbon, ſince the product of the union has the ſame properties.

The converſion into ſteel is not doubtful. The ingot having been poliſhed on a lapidary's wheel, a drop of weak nitrous acid immediately produced a dark-grey ſpot, abſolutely like that exhibited on Engliſh caſt ſteel, and on caſt ſteel produced by the proceſs of C. Clouet. Thoſe who have often tried ſteel by this kind of proof, long ago pointed out by Rinmann, had occaſion to remark, that the ſpot of caſt ſteel, though very ſenſible, is however leſs black than that of ſteel made by cementation, which depends perhaps on the different degree of oxydation of the carbon which they have taken in.

Explanation of the FIGURES.

A, fig. 3, (Plate II.) is the plan of the iron crucible. B, a ſection of this crucible. C, fig. 4, the ſtopper of the crucible. D,E, fig. 5, ingot of caſt ſteel ſeen in perſpective. The ſpot formed by the nitric acid on the poliſhed part is repreſented at *a*.

* Some perſons having expreſſed a deſire to ſee the inſide of the ingot, it was broken on the anvil, which was not effected without ſeveral blows from a very large hammer. It divided itſelf into two fragments, which were exhibited at the next ſitting. The fracture appeared perfectly uniform, and of the moſt beautiful grain.

XIX. *Ninth*

XIX. *Ninth Communication from Dr.* THORNTON, *Physician to the General Dispensary, relative to Pneumatic Medicine.*

A DISEASED LIVER CURED BY VITAL AIR.

THE butler of Colonel Ironside, who had been long resident in India, laboured for several years under a well-marked liver complaint. He had been under the care of Dr. Warren, and other physicians, without experiencing any essential advantage. Colonel Ironside, as all other means had been ineffectual, wished him to try the vital air. He accordingly came under my care, and the same tonics were employed as he had before taken; therefore I ascribe the cure to the vital air, which was conjoined with these, and which soon completely restored him to health; and he has continued well now above a twelvemonth.

Observations.

When animals placed in pure vital air were destroyed by so powerful a stimulus, Dr. Beddoes found the liver not liver-coloured, but of a florid red. We therefore can easily suppose it to act on this organ when given in a moderate way: and as the oxyds of mercury and the nitrous acid, which are the best remedies for this disease, act chiefly from their contained oxygen, it is probable that the vital air will hereafter be found a specific in this complaint, possessing superior advantages over both these remedies, and will supercede them, although it cannot be put up into two-ounce phials, the principal objection raised against it; for the airs can now (which originated from my suggestion) be confined in barrels, and bottled off as easily as wine: and I must observe, that this patient had a barrel of vital air, containing 24 gallons, which cost him one guinea, conveyed for him to his master's seat in the country, and a tin pneumatic apparatus for inhaling the medicinal air, which stood him at the low rate of thirty shillings; which improvements, I trust, will greatly facilitate the general application of pneumatic medicine, when it will be sure to find that level its merits entitle it to.

INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

ELECTORAL ACADEMY OF SCIENCES AT MANHEIM.

ON the 16th of April laſt the Electoral Academy of Sciences at Manheim held a public ſitting in commemoration of its deceaſed founder the Elector Charles Theodore, who died on the 17th of February. On this occaſion Mr. M. Collini, a member of the Society, read a printed oration on the viciſſitudes of the Academy. The moſt flouriſhing period of this inſtitution was that between the time of its eſtabliſhment and the year 1777, when the Palatinate family got poſſeſſion of Bavaria, and Charles Theodore transferred his reſidence from Manheim to Munich. It was not, however, left deſtitute of ſupport; but its diſtance from the electoral court rendered many things impoſſible, which, under other circumſtances, might have been accompliſhed. Still more prejudicial to the Academy, as well as the whole Palatinate, was the war which broke out afterwards, between France on the one ſide, and the German Empire and Auſtria on the other. Manheim was bombarded three times, *viz.* in December 1794, November 1795, and January 1798; and as often was it neceſſary to remove the library of the Academy, as well as its papers, &c. to a place of ſafety, and to arrange them. The ſittings and labours of the members were interrupted; the palace, aſſigned to them for holding their meetings, was in part burnt; and as the revenues deſtined for the ſupport of the inſtitution ariſe from poſſeſſions on the left bank of the Rhine, if theſe remain in the hands of the French, it is to be apprehended, that, like many other eſtabliſhments of the ſame kind on the right bank of that river, which had their revenues on the other ſide, it will decline unleſs ſupported by the new regent. Among its moſt diſtin-

guished members, since the time of its foundation, may be reckoned Voltaire, elected in 1764; Lessing, and Lalande the celebrated French astronomer. It was established in 1763 according to a plan of the learned Schœpflin, and divided into two classes, the Historical and Physical; the latter of which, in the year 1780, was subdivided into the Physical (properly so called) and Meteorological. The collection of the papers of the Academy have been published in eleven volumes quarto, under the title of *Acta Academiæ Theodoro-Palatinæ.* The Meteorological Observations are from 1781 to 1792, and make twelve volumes quarto, with the title of *Ephemerides Societatis Meteorologicæ Palatinæ.*

SUGAR FROM BEET-ROOTS.

Professor Klaproth has published the following testimony in regard to sugar procured from beet-roots:

"Having, in consequence of a request by Mr. Achard, that I would examine his process for making sugar from beet-roots, and communicate to him the result, convinced myself, by repeated experiments, of the abundance of saccharine matter in these roots, that I might ascertain the proportion with more accuracy, I made the following experiment:—Twenty-five fresh roots, which weighed 32½ pounds after they had been scraped and the tops cut off, were put into a perforated tin vessel, and the juice was expressed by a proper apparatus. The juice obtained weighed 19½ pounds. The squeezed residuum was put into a tin kettle with boiling water, and, after standing an hour, was pressed also. This extract was added to the juice; both were boiled to a syrup, and strained through a woollen cloth; after which the syrup was put into a porcelain capsule, and evaporated gradually over a slow fire to complete dryness. The raw sugar obtained, and which I put into a sealed glass, amounted to two pounds twelve ounces.

"That 25 roots, or 32½ pounds of beets, produced two pounds twelve ounces of sugar, is hereby certified, as witness my hand, MARTIN HENRY KLAPROTH."

"Berlin, 11th January 1799."

A letter

A letter from Berlin, dated September 28, states, that the commission appointed by his Prussian majesty, to examine Achard and Klaproth's process for making sugar from beet-roots, have finished their report on that subject: The result is, that 1500 pounds of beet-root gave 398 pounds of very agreeable syrup, which produced 57 pounds of powder-sugar of a white colour, and proper for use without being any farther refined.

INDIGO RESIN.

Professor Brugnatelli, of Pavia, has lately announced, that, by treating indigo with the nitrous acid, one may obtain a large quantity of a peculiar resin, which he calls *Resina indigofera*. An ounce of indigo distilled with four ounces of the nitrous acid, left a thick mass, in which he found a concrete substance of a red colour, which, when separated, had all the properties of resin: it weighed half an ounce. This resin dissolved very easily in alcohol, which acquired from it a dark red colour. Water decomposed this solution immediately, and produced a yellow precipitate. This tincture communicated its colour to paper and linen, and gave it a beautiful and durable yellow dye. It communicated the same colour to the skin and the nails: the former retained its colour for a long time. This resin has a very bitter, astringent, and disagreeable taste; but no sensible smell. On glowing coals it melts and burns with a flame, and at the same time emits a sharp disagreeable smell.

CONVERSION OF IRON INTO STEEL.

Our valued correspondent Mr. Mushet, of the Clyde Iron-Works, has examined and repeated C. Clouet's process with considerable success. The experience of Mr. Mushet in similar processes rendered him well qualified for the investigation, and the result has been what we should have promised from our knowledge of his abilities and accuracy. Mr. Mushet, instead of experiencing any loss in the weight of iron employed, has uniformly obtained the same increase gained in the large way of manufacture by the common process of cementation with charcoal, *viz.* from $\frac{1}{7.3}$ to $\frac{1}{7}$th. However, in varying the experiments of the French chemists, he has

met with phenomena that force him to draw very different conclusions from them, and to reject the idea of the carbon being furnished by the decomposition of the carbonic acid; for, in some experiments, he employed for the cementation calcareous earth, previously deprived of its acid, and excluded the contact of external air, and the result was cast steel. These experiments were performed in crucibles made of Sturbridge clay, without any mixture. Superior effects, however, were produced in half the time, by presenting carbon, in a comparatively deoxydated state, in crucibles variously compounded. Some curious questions naturally arise from the result of these experiments; especially from the first, as, Whence comes the carbon? But as Mr. Mushet has promised, in some future Number, to lay his experiments on this interesting subject before the public, our philosophical readers will then be better enabled to enter upon the investigation.

IMPROVED WRITING INK.

C. Van Mons has applied the discoveries of Proust to the preparation of common writing ink. He has found that the sulphat of iron, calcined to whiteness, always gives a most beautiful black precipitate. By the following mixture he obtained excellent ink:—Galls, 4 ozs.; sulphat of iron, calcined to whiteness, $2\frac{1}{2}$ ozs.; and two pints of water. The whole must be left to infuse cold for 24 hours; then add gum Arabic 10 drachms, and preserve it in a stone jar, either open, or covered merely with paper.

MANGANESE IN VEGETABLES.

M. Proust has lately published the following short way of proving the presence of manganese in such vegetable productions as contain any; which has this to recommend it, that it is shorter and more effectual than that of Scheele. Put in the process distilled vinegar (employing heat) on well-washed ashes, without attempting, however, to extract from them all that they are capable of yielding to the acid. The liquor then contains oxyd of manganese, lime, and magnesia. Try it by the prussiat of pot-ash, and it gives a precipitate of the colour of peach blossoms, which, treated by the blow-pipe in the usual manner,

manner, gives constantly the colour which denotes the presence of that oxyd. This precipitate is not without a little iron, which it receives, as I believe, from the prussiat of potash. Then put nitric acid over the residuum of the ashes, and it will take from them the iron, which may be proved also by the prussiat. Then search for the different earths, which may be discovered by the common means. The ashes of the pine, calendula, vine, green oak, and fig-tree, contain manganese. The ashes of the fig-tree are almost all siliceous; the ashes of barilla do not contain an atom of it, but iron in great quantity, magnesia, &c.

PREPARATION OF STUFFS FOR DYEING.

A letter from Van Mons to Brugnatelli, printed in the 92d number of the Annales de Chimie, contains a useful hint on this subject. He expresses himself as follows:

"The memoir, read by Giobert to the Academy of Turin on the animalisation of flax and cotton, induces me to mention a circumstance which ought to have led me to the discovery of this animalisation long ago, had I paid proper attention to it at the time. I had caused some oyster-shells to be boiled with pot-ash and lime, in order to whiten them for some pharmaceutical purposes; and I employed the ley produced by that operation, to bleach the linen cloths used to strain the decoctions. My pupils made me afterwards observe, that these pieces of cloth were strongly dyed when any substances were passed through them, the colour in which might have been carried off by merely washing in cold water."

YELLOW DYE FROM A SPECIES OF MUSHROOM.

Among the different kinds of mushrooms, capable of producing lively and durable dyes, none is more worthy of notice than the *Boletus hirsutus* of Bulliard, from which C. Lasteyrie has extracted a bright, shining, and very durable yellow dye. This pretty large mushroom grows commonly on walnut and apple-trees. Its colouring-matter is contained in abundance, not only in the tubular part, but also in the parenchyma of the body of the mushroom. In order

to extract it the muſhroom is pounded in a mortar, and the liquor thence obtained is boiled for a quarter of an hour in water. An ounce of liquor is ſufficient to communicate colouring-matter to ſix pounds of water. When the liquor has been ſtrained, the ſtuff to be dyed is put into it, and boiled for a quarter of an hour. All kinds of ſtuff receive this colour and retain it, but on linen and cotton it is leſs bright. This colour may be modified, in a very agreeable manner, by the effect of mordants.

The proceſs ſucceeded beſt on ſilk. When this ſubſtance, after being dyed, is made to paſs through a bath of ſoft ſoap, it acquires a ſhining golden-yellow colour, which has a perfect reſemblance to the yellow of that ſilk employed to imitate embroidery in gold, and which has hitherto been brought from China and ſold at a dear rate, as the method of dyeing it is unknown in Europe. The yellow colour extracted from this muſhroom may be employed alſo with advantage for painting in water-colours as well as in oil.

NATURAL HISTORY.

The two following articles on the Friendſhip and the Sagacity of Birds, by Mr. Simpſon of New-York, we copy from the Medical Repoſitory, a valuable work publiſhed in America:—

" Mr. Myers, a brother-in-law of mine, moving from Wilton to Philadelphia, deſired me to ſend for a large turkey-cock and hen, and a pair of bantams, which had been a long time in his yard, and which the family did not chooſe to have killed. Accordingly, after his departure, I had them brought home, and put with ſome other poultry that were then running in my yard. Some time after, as I was feeding the poultry from the barn-door, a large hawk turned the barn, and ſuddenly made a pitch at the bantam hen: ſhe immediately gave the alarm, by a noiſe which they generally make on ſuch occaſions; when the large turkey-cock, who was about two yards diſtance, and who I preſumed ſaw the hawk's intentions, and the imminent danger of his old acquaintance, flew at the hawk with ſuch violence, and gave him ſuch a ſevere ſtroke with his ſpurs as he was going to

ſeize

ſeize his prey, as to knock him from the hen to a conſiderable diſtance; and the timely aid of this faithful auxiliary, the turkey-cock, ſaved the bantam from being devoured by the hawk."

During my reſidence at Wilton, early one morning I heard a noiſe from a couple of martins, who were jumping from tree to tree adjoining my dwelling. They made ſeveral attempts to get in a box or cage fixed againſt the houſe, which they had before occupied; but they always appeared to fly from it with the greateſt dread, and repeated thoſe loud cries which firſt drew my attention. Curioſity led me to watch their motions. After ſome time a ſmall wren came from the box or cage, and perched on a tree near it, when her ſhrill notes ſeemed to amaze her antagoniſts. After ſome time ſhe flew away. The martins took this opportunity of returning to their cage: but their ſtay was ſhort. Their diminutive adverſary returned, and made them fly with the greateſt precipitation. They continued manœuvring in this way the whole day, and I believe the wren kept poſſeſſion during the night. The following morning, on the wren's quitting the cage, the martins immediately returned, took poſſeſſion of their manſion, broke up their own neſt, which conſiſted of twigs of different ſizes; went to work, and with more induſtry and ingenuity than I ſuppoſed they poſſeſſed, they ſoon barricaded their doors. The wren returned, but could not re-enter. She made attempts to ſtorm the works, but did not ſucceed.—I will not preſume to ſay that the martins followed our modern maxim, and carried with them a ſufficiency of food to ſuſtain a ſiege, or that they made uſe of the abſtinence which neceſſity ſometimes, during a long and bad ſtorm, might probably occaſion; but they perſevered for near two days, to defend the entrance within the barricado; and the wren, finding ſhe could not force an entry, raiſed the ſiege, quitted her intentions, and left the martins in quiet poſſeſſion without further moleſtation.

DEATHS.

On Friday, the 11th of October, Samuel More, Eſq. who for many years paſt filled the office of Secretary to the Society

for the Encouragement of Arts, Manufactures, and Commerce. The assiduity and ability with which he discharged the duties of the office he held, for that respectable Institution, will make it difficult to find another to fill it who may be as well qualified.

A few weeks ago, at New-York, Dr. Perkins, inventor of the metallic tractors. He had gone thither, from his usual place of residence in Connecticut, to assist his brother practitioners in endeavouring to arrest the progress of the yellow fever, with which that city has been again visited, and caught the infection, which carried him off.

In the Philosophical Magazine for April last we announced the death of that celebrated naturalist Spallanzani on the 11th of February. It appears, however, that he died on the 17th; and, as every thing that relates to the last moments of eminent men is interesting, we hope our readers will not be displeased with us for adding the following particulars:—On the evening of the 10th he was seized with an apoplectic fit, the consequence of a neglected strangury. Professors Brera and Scarpa, who were called in to visit him, immediately saw that all medical aid would be useless; for, on inspecting his urine, it was found that the bladder was in a state of gangrene. They however ordered him such medicines and treatment as they thought could be of any service. On the 12th the affection in the left arm and the lips disappeared. On the 13th he recovered the full use of his senses; found himself much better; and, without apprehending death, attended to his literary and domestic concerns. Early on the morning of the 17th he was attacked by a general debility, accompanied with a diarrhœa entirely colliquative. The two professors then told him, that this day would be the last of his life; an information which Spallanzani received with the greatest firmness of mind. Towards noon he embraced his friends, the two professors, and particularly his brother, who had come express to Pavia from Scandiano, in the duchy of Modena; took the most affectionate leave of them, and exactly at 11 o'clock breathed his last in their arms. The opinion of the physicians respecting his disease was confirmed by the opening of his body. A history of it has been published by

Professor

Professor Brera, who was two years physician to this eminent naturalist. Two monuments are to be erected at the public expence to his memory; one in the Pantheon of Milan, and another within the precincts of the University of Pavia.

In the same Number of the Philosophical Magazine we announced also the death of Professor Lichtenberg, of Gottingen, on the 24th of February. Since that period a small work has appeared at Gottingen under the title of, *Elogium Georgii Christophori Lichtenberg, in concessu Soc. Reg. Scientiarum recitavit A. G. Kastner, 20mo. d. Ap.* 1799; from which we extract the following particulars:—Professor Lichtenberg was born on the 1st of July 1744, at Oberamstadt, in Hesse Darmstadt. He was of a weakly constitution from his birth, and applied himself very early to the mathematics and natural history, in which he was greatly assisted by his two brothers, who were older. In the year 1763 he went to Gottingen, where he became the pupil and friend of Kastner and Meister, and attended the Lectures of the other professors. In conjunction with Erxleben he made some observations on the earthquake of 1767; and remarked, in particular, that the shock continued six seconds; while others, not accustomed to mathematical accuracy, extended its duration to a whole minute. He delineated the courses of the comets which had been observed; and it is remarked, that he possessed a great deal of patience for works of this kind. Professor Kastner has now in his possession a drawing by him, of the face of the moon, where the spots are marked in the order in which they enter the earth's shadow during eclipses. In the year 1770 he was invited to Giessen; but he preferred Gottingen, where he was appointed Professor Extraordinary. In the years 1772 and 1773 he was commissioned to determine, from astronomical observations, the position of various places in his Britannic Majesty's German States; and for this purpose was supplied, at the king's expence, with a quadrant by Sisson. In 1776 he laid the result of this labour before the Royal Society, of which he had been elected a member two years before. As repeated wishes had been expressed that Mayer's manuscripts might be published, Professor Kastner put them into the hands of Lichtenberg, who, assisted by Dietrich, published

published the first volume, in 1775, with a typographic elegance which had been before unknown in Germany. Want of leisure prevented him from editing a second. Lichtenberg was twice in England; the first time in 1770, and again in 1774. In the year 1777, after Erxleben's death, he studied natural philosophy, in the introduction to that science written by his predecessor; the four last editions of which were published under his inspection, but always with additions by himself. On this occasion he made a collection of philosophical instruments, which afterwards became the property of the University. Among the new experiments, which were peculiarly Lichtenberg's own, is that respecting the figures formed by pounded resin on the electrophore, by which he examined and traced out the nature and progress of the electric fluid in a new manner, and communicated the result of his researches in two papers, read before the Royal Society in 1777 and 1778. Professor Lichtenberg had a very extensive correspondence, which enabled him to communicate to the Society a variety of useful intelligence. He proposed, alternately with Kastner, the prize questions of the mathematical class, and was the author of the last for the present year, respecting the motion of steam in conductors of a given kind. In the year 1780 he began, in conjunction with the late J. R. Forster, the Gottingen Magazine, six numbers of which appeared annually, till 1785, when it was dropped with the first number of that year. In the Gottingen Pocket Calendar, of which he was the editor for many years, he published various articles equally useful and agreeable. Among these was an account of Hogarth's satyrical prints, accompanied with engravings. The large account of these prints, with a commentary, is well known. As Professor Lichtenberg's complaints returned regularly every year, the last attack was at first not considered dangerous; but an inflammation of the lungs taking place, he was carried off on the 24th of February, as before stated.

THE

PHILOSOPHICAL MAGAZINE.

NOVEMBER 1799.

I. *Obfervations on the Flux and Reflux of the Atmofphere. By the Abbé* MANN *.

AS the general power of attraction or gravity extends to all bodies within the fphere of its action; and as the atmofphere of our earth confifts not only of heavy, but alfo of moveable and elaftic parts, which revolve around a common axis like the water of the ocean, it neceffarily follows that the atmofphere muft be affected by the fame phyfical caufes which produce the flux and reflux of the fea. This confequence is fo immediate and neceffary that it is not poffible to doubt it: the queftion therefore will not be the exiftence of a flux and reflux, but merely refpecting their extent. Many philofophers have afferted, on grounds which do not appear to have much validity, that the flux and reflux of the atmofphere amount to no more than thofe of the ocean, that is, eight feet. According to their opinion, a fea of water, air, or quickfilver, would be raifed to about the fame height by the effects of the fun and moon, and acquire from them nearly the fame movement. Thofe who fpeak in this manner, muft entirely overlook the nature of elaftic fluids; for it may eafily be conceived that, with the fame power of at-

* From the *Tranfactions of the Academy of Sciences at Bruffels*, Vol. IV.

 traction,

traction, the movement of an elaſtic fluid would be totally different from that of a non-elaſtic. Other philoſophers, not neglecting this conſideration, have believed that the cauſes which produce the movement of the waters of the ocean can occaſion a much greater in the aërial maſs of the atmoſphere, and muſt have a very great ſhare in producing the regular winds, and other phenomena which depend on them.

D'Alembert has calculated, from the theory of general gravitation, thoſe movements which muſt be produced in the atmoſphere by the effects of the ſun and moon. He found that from this influence a continual eaſt-wind muſt ariſe at the equator, and that, in the temperate zones, not far from the tropics, it muſt be converted into a weſt-wind; that on account, however, of many local circumſtances, and other impediments which come in the way, it cannot always continue in the ſame direction; and that the variations in the ſtate of the barometer, which thence ariſe, muſt be very inconſiderable, and almoſt imperceptible.

Bacon, Gaſſendi, Deſchales, Goad, Dampier, Halley, and others who have written on the wind, all unanimouſly obſerve, that the periods of the year moſt expoſed to it are the two equinoxes: that ſtorms are moſt frequent at the times of new and full moon, and particularly thoſe which happen near the equinoxes: that at periods, otherwiſe calm, a ſmall breeze always takes place at high water; and that a ſmall movement in the atmoſphere is each time perceived a little after noon and midnight. Now, as moſt of theſe circumſtances have a great reſemblance to thoſe which accompany the flux and reflux of the ſea, and take place at the ſame time; and as in this reſpect the movement of water and the atmoſphere is the ſame, according to Newton's rule, that natural effects of the like kind muſt be produced by the ſame cauſes *, both may be deduced from the ſame ſource.

From this it appears that theory and experience coincide to eſtabliſh the exiſtence of an atmoſpheric flux and reflux, and of the regular and very perceptible effects which they produce. Nothing therefore is neceſſary but to deduce their

* Effectuum naturalium ejuſdem generis eædem aſſignandæ ſunt cauſſæ.

phenomena

phenomena and extent from the well-known properties of elastic fluids. For this purpose we must have before our eyes the following principles of aërometry, to be found in most elementary books:—

1. The elasticity of fluids is in the inverse ratio of their density, and in the direct ratio of their rarity. Thus air is more elastic than water; light more elastic than air; and ether than light. This is one of the principles of Newton *.

2. The force or elasticity of the air expands and contracts itself in the direct ratio of the weight with which it is loaded, and diffuses itself in the inverse ratio of the force by which it is compressed †.

3. The air is rarefied, or diffuses itself, in the direct ratio of the quantity of heat which acts upon it ‡.

4. The air, as well as all fluids in general, has a tendency to put itself in equilibrium, and does not rest until it has obtained it §.

It now follows, from these principles, that every thing which increases the weight of the atmosphere, and presses the air more in one place than another near it, must occasion the air to rush from the former to the latter, where it is lighter: and, on the other hand, every thing that lessens the gravity of the atmosphere, and increases or rarefies the air, and makes it lighter than in the neighbouring parts, must occasion the air to rush in from all sides to those parts; and this continues in each case till the equilibrium is restored, and rest again effected. This is the principal cause of the origin of wind. But, as a pendulum put in motion does not immediately rest when it has come to a perpendicular direction, but obtains rest after making some small vibrations; in the same manner, the currents of air, from the places where the air is more pressed to that where it is less so, will rush beyond the boundaries of the equilibrium, from which they will again fly back, and at length obtain rest after several undulations. As this takes place in all fluids, it is more

* Princip. in fine, p. 530, edit. 1726, et quæst. 21, 22, in fine optices.
† Wolf Elem. Aërom. § 72—77. ‡ Ibid. § 146. § Ibid. § 36—44.

peculiarly common to elaſtic fluids; and it is obſerved, according to theſe principles, that the winds during ſtorms change their courſe, and blow exactly from thoſe points to which the current was before directed.

The ſpecific gravity of the air is 800 times leſs than that of water, and its elaſticity is infinitely greater. The attractive power of the ſun and moon leſſens the gravity and preſſure of the atmoſphere towards the earth, as it draws it towards theſe bodies in the ſame manner as it does the water of the ocean. But this power of attraction does not extend the water of the ocean, which is deſtitute of elaſticity, as it does the whole maſs of the atmoſphere (poſſeſſed of it, and expoſed to the action of this power) in the ratio of the decreaſe of gravity, and of the preſſure of the atmoſphere towards the earth. Adopting, therefore, the inadmiſſible ſuppoſition, that water, air, and quickſilver would be raiſed to nearly the ſame height, and be moved in the ſame manner by the attractive force of the ſun and moon; this much at any rate is proved, that the extenſion of the air, in proportion to the leſſening of the gravity of the atmoſphere towards the earth, muſt always take place, and produce a conſiderable atmoſpheric flux, while this effect cannot occur in regard to the flux of the ocean. It is certain that fluids are more or leſs ſubject to the effects of any power acting upon them, according as they have more or leſs mobility, which is in the direct ratio of their rarity and elaſticity. Now, as air poſſeſſes theſe properties in a degree 800 times greater than water, the flux of the atmoſphere muſt very perceptibly exceed in magnitude that of water. This, therefore, is a ſecond certain argument againſt thoſe who pretend that the flux and reflux of the atmoſphere are ſimilar to thoſe of the ocean. Not only muſt the attractive power of the ſun and moon, on account of the greater elaſticity and mobility of the air compared with water, produce a greater effect on the atmoſphere than on the ocean, but the component parts of the atmoſphere, as they are nearer the moon by about a ninetieth part of the ſemidiameter of the earth, muſt be more ſtrongly attracted than the watery particles of the ocean; in the ſame manner as thoſe which are 90 degrees

diſtant

diſtant from the place where the moon is vertical, muſt occaſion a greater preſſure on the earth on account of the greater obliquity of the attraction, and this in proportion to the greater height of the atmoſphere above that of the ſea. This is the third phyſical cauſe which makes the flux and reflux of the atmoſphere more conſiderable than that of the ocean. To this we may add the immenſe ſpace occupied by the atmoſphere in compariſon of the ocean; for if the whole globe were covered with water, this general ocean would not occupy the fiftieth part of the ſpace actually filled by the atmoſphere; and in my opinion the extenſion effected in elaſtic fluids, by the ſame power of attraction, is in proportion to their maſſes. Suppoſing this to be the caſe, the flux of air in an atmoſphere of ten miles height, would be much more conſiderable than in another amounting to only a tenth part of that height: though this difference cannot take place in the flux and reflux of the ocean, becauſe water is deſtitute of elaſticity. Beſides, lands, iſlands, ſtraits, bays, the ſituation of the coaſts, ſand-banks, ſhoals, &c. throw a great many impediments in the way of the water in regard to its motion; whereas the atmoſphere, which riſes to a height ten or twelve times greater than the ſummits of the higheſt mountains, is in no manner prevented from moving according to the effects of the power of attraction, unleſs ſomething very particular takes place in its loweſt ſtrata. It is more than probable that each of theſe cauſes contributes its part to make the movement of the atmoſphere far more conſiderable than that of the ocean.

It is therefore certain that the united effects of the attraction of the ſun and moon on the atmoſphere of the earth muſt raiſe and extend it in the inverſe ratio of the ſquare of the diſtance ſo as to make it aſſume the form of a lengthened ſpheroid, the greater diameter of which, from the ſame grounds and with the ſame variations as the aqueous ſpheroid, muſt nearly follow the direction of the line of attraction.

Beſides, the heat of the ſun, which has no ſenſible influence in raiſing or extending the water of the ocean, will produce an effect on that part of the atmoſphere expoſed to its

rays

rays by heating and rarefying it, in the ratio of its ſtrength, according to the third of the before-mentioned principles. That part, therefore, of the atmoſphere which is gradually turned towards the ſun, muſt extend in proportion to the degree of heat which acts upon it, and riſe above the reſt; and this atmoſpheric ſwelling will conſtantly follow the apparent daily courſe of the ſun. The moon, on the other hand, has no power to produce or change this phenomenon; becauſe, in all the experiments hitherto made to collect and concentrate its rays, the ſmalleſt degree of heat has never been perceived.

As there are therefore two different cauſes, *viz.* the united attraction of the ſun and moon, and the heat of the ſun alone; by which the latter, except at the time of the ſyzegies, when they both act in the ſame direction upon one point, has a particular influence on the atmoſphere of the earth independent on that of the former, it thence follows that they will produce three different fluxes every day. Two of them ariſe from the attractive power of the ſun and moon, and in their formation, direction, and movement, are perfectly ſimilar to thoſe produced in the ocean from the ſame cauſes. The third, however, is produced by the heat of the ſun alone; and its prominent parts will always be in that parallel through which the ſun paſſes in the courſe of his daily movement, and will continually follow that luminary, from parallel to parallel, at a ſmall diſtance. The two firſt I ſhall call *attraction-tides*, the third *heat-tides*.

The tides of attraction, like thoſe of the ocean, and from the like grounds, have at the ſame time, at two oppoſite ends of the globe, projecting parts, and theſe lie almoſt in that line which might be drawn from the centre of the earth to that of the moon. The heat-tides, on the other hand, can take place only on one point of the globe; that is, in the point to which the ſun is vertical. Their projecting part will be directed towards that luminary, and nearly follows its movement.

In regard to the effects of theſe atmoſpheric tides, they depend on the natural ſtate of the aërial fluid, and on its reſt, which conſiſts in an equilibrium of all its parts. When this

this equilibrium is deſtroyed, the atmoſphere returns to its former ſtate as ſoon as the cauſe of this derangement is removed. If this equilibrium is by any cauſe deſtroyed in any one place, a movement muſt naturally follow from all ſides, and continue until the equilibrium is again reſtored; and in this we may diſcover the principal cauſe of the origin of wind. Every thing, therefore, which can derange the equilibrium of the atmoſphere in any manner, muſt be reckoned among thoſe cauſes which give riſe to wind: ſuch as the flux and reflux of the atmoſphere, occaſioned by attraction and heat; all other rarefaction of the air by heat; or the condenſation of it by cold, &c.; the letting looſe of vapours by fermentation and evaporation into the atmoſphere; the eruption from the boſom of the earth and aſcent of elaſtic vapours, volcanoes, earthquakes, and perhaps other cauſes not ſo evident, and therefore leſs known. The exceedingly variable directions of the wind depend on the nature of the countries from which they blow; and, in particular, on mountains, foreſts, rivers, marſhes, lakes. In a word, every thing that can oppoſe or favour the free movement of the aërial fluid has an influence on the direction of the wind.

Among all thoſe cauſes which derange the equilibrium of the atmoſphere, and contribute to the production of wind, the principal and moſt uniform is the rarefaction and condenſation of the air. Both are the immediate effect of the different atmoſpheric tides. The regular courſe of theſe tides from eaſt to weſt, in conſequence of that of the ſun and moon, muſt produce a continual eaſt wind; which is however exceedingly weak, but continues without interruption, and prevails only in the immenſe ſeas of the Torrid zone, though with moſt regularity in the Pacific ocean, on account of its vaſt extent. The compreſſed parts of the atmoſphere in the neighbourhood ruſh towards thoſe moſt rarefied by the atmoſpheric tides, and follow them alſo in their regular progreſs from eaſt to weſt. The parts of the atmoſphere, however, in the weſt, will have a much leſs, or perhaps no movement towards thoſe which proceed from the eaſt, on account of the atmoſpheric flux moving towards them: at any rate, the motion from eaſt to weſt will exceed that ſmall motion

from weſt to eaſt, and therefore produce a continual eaſt wind in the parallel of the atmoſpheric tide. A wind alſo will proceed from the north or ſouth of this parallel, in an oblique direction north and ſouth, to the diſtance of about thirty degrees on each ſide of the equator. In both the temperate zones the regular winds muſt blow from weſt to eaſt within theſe boundaries, as they incline towards the parallel of the atmoſpheric tide, and again reſtore the equilibrium of the atmoſphere, which has been rareſied by the continual movement of the aërial tides. Theſe conſequences, deduced from theory, coincide perfectly with the courſe of the wind in the ocean. At land, and on confined ſeas, many other cauſes exiſt which contribute to change the regular direction of the winds, and to make them variable.

On the coaſts, in the torrid zone, the wind blows for the moſt part from the ſea towards the land. The reaſon of this is evident. The reflected rays of the ſun, and other cauſes, heat and rarefy the air at land much more than at ſea; and the direction of the wind always proceeds towards the rarefied part of the atmoſphere. The places where the laſt mentioned winds ſeparate themſelves from the regular winds, muſt be abſolutely calm. All this is confirmed by experience throughout the whole torrid zone, and particularly on the coaſt of Guinea.

Beſides the determination of the courſe of the winds in conſequence of the atmoſpheric flux and reflux, there are an immenſe number of local and accidental cauſes which have an influence on the creation and direction of the winds. One of the leaſt irregular is the preſſure and gravity of the atmoſphere in the frigid zones, which muſt occaſion a continued movement of the air towards the more rareſied parts, and at the ſame time a wind from the poles towards the equator. As ſoon, however, as this continual wind enters the temperate zones, its direction will be overcome and deranged by other local cauſes: an examination, however, of thoſe winds not ariſing from the atmoſpheric tides, does not fall within the plan of this eſſay.

Thoſe winds, on the other hand, which generally blow at ſun-riſe and ſun-ſet, are a remarkable effect of the atmoſpheric

ſpheric tides produced by heat, as the gentle wind obſerved at the times of high water in the ocean, even during weather in other reſpects calm, proceeds from an atmoſpheric tide produced by attraction.

The difficulty of breathing which people experience in the torrid zone, under an atmoſpheric tide, ariſing from heat, muſt be aſcribed as much to a rarefaction of the air thereby occaſioned, as to the heat itſelf. A like effect, in regard to breathing, is perceived on high mountains; which however ariſes from the thinneſs of the air on theſe eminences, for here the heat can have no part in the rarefaction.

It happens ſometimes, in warm countries, that during the atmoſpheric tides, the heat and rarefaction of the air increaſe to ſuch a degree that they create ſcorching and ſuffocating winds, known under the name of *Sclanos*; the violence of which proceeds ſo far ſometimes, that they deſtroy on the ſpot thoſe who are immediately expoſed to them. Abundant and horrid inſtances of this circumſtance are quoted by the Abbé Richard in his Natural Hiſtory of the Air and Meteors which have occurred in the Deſarts of Arabia, in the neighbourhood of the Perſian Gulph, and other places.

Beſides theſe *ſolanos*, it is obſerved alſo, ſometimes, that the wind ſuddenly ruſhes from all quarters to that point where the air is moſt rarefied, which occaſions ſtorms and hurricanes in that part by the mutual ſhock of the air and the vapours ſtreaming in all directions towards one centre, and the winds then blow backwards again from this point to every quarter of the heavens till the equilibrium is reſtored. This effect, ſo natural and common, has long aſtoniſhed thoſe who never employed themſelves in endeavouring to diſcover the cauſe.

In the laſt place, there is good reaſon for conjecturing that the ſame phyſical cauſes which produce the different atmoſpheric tides, with their conſequences, contribute alſo to change the weather and temperature, and to produce at the ſame time a great many other meteorological phenomena.

II. *On the Nature and Construction of the Sun and Fixed Stars.* *By* WILLIAM HERSCHEL, *LL.D. F.R.S.**

AMONG the celestial bodies, the Sun is certainly the first which should attract our notice. It is a fountain of light that illuminates the world! It is the cause of that heat which maintains the productive power of Nature, and makes the earth a fit habitation for man! It is the central body of the planetary system; and what renders a knowledge of it still more interesting to us is, that the numberless stars which compose the universe, appear, by the strictest analogy, to be similar bodies. Their innate light is so intense, that it reaches the eye of the observer from the remotest regions of space, and forcibly claims his notice.

Now, if we are convinced that an enquiry into the nature and properties of the sun is highly worthy of our notice, we may also, with great satisfaction, reflect on the considerable progress that has already been made in our knowledge of this eminent body. It would require a long detail to enumerate all the various discoveries which have been made on this subject; I shall therefore content myself with giving only the most capital of them.

Sir Isaac Newton has shewn that the sun, by its attractive power, retains the planets of our system in their orbits: he has also pointed out the method whereby the quantities of matter which it contains may be accurately determined. Dr. Bradley has assigned the velocity of the solar light with a degree of precision exceeding our utmost expectation. Gallileo, Scheiner, Hevelius, Cassini and others have ascertained the rotation of the sun upon its axis, and determined the position of its equator. By means of the transit of Venus over the disk of the sun, our mathematicians have calculated its distance from the earth; its real diameter and magnitude; the density of the matter of which it is composed; and the fall of heavy bodies on its surface.

* From the *Philosophical Transactions of the Royal Society for* 1794, Part I.

From

From the particulars here enumerated it is fufficiently obvious, that we have already a very clear idea of the vaft importance and powerful influence of the fun on its planetary fyftem: and if we add to this the beneficent effects we feel on this globe from the diffufion of the folar rays; and confider that, by well-traced analogies, the fame effects have been proved to take place on other planets of this fyftem, I fhould not wonder if we were induced to think that nothing remained to be added in order to complete our knowledge: and yet it will not be difficult to fhew that we are ftill very ignorant, at leaft with regard to the internal conftruction of the fun. The various conjectures which have been formed on this fubject, are evident marks of the uncertainty under which we have hitherto laboured.

The dark fpots in the fun, for inftance, have been fuppofed to be folid bodies revolving very near its furface. They have been conjectured to be the fmoke of volcanoes, or the fcum floating upon an ocean of fluid matter. They have alfo been taken for clouds. They were explained to be opaque maffes fwimming on the fluid matter of the fun, dipping down occafionally. It has been fuppofed that a fiery liquid furrounded the fun, and that, by its ebbing and flowing, the higheft parts of it were occafionally uncovered, and appeared under the fhape of dark fpots; and that, by the return of the fiery liquid, they were again covered, and in that manner fucceffively affumed different phafes. The fun itfelf has been called a globe of fire, though perhaps metaphorically. The wafte it would undergo by a gradual confumption, on the fuppofition of its being ignited, has been ingenioufly calculated: and, in the fame point of view, its immenfe power of heating the bodies of fuch comets as draw very near to it has been affigned.

The bright fpots, or faculæ, have been called clouds of light, and luminous vapours. The light of the fun itfelf has been fuppofed to be directly invifible, and not to be perceived unlefs by reflection; though the proofs which are brought in fupport of that opinion feem to me to amount to no more than, what is fufficiently evident, that we cannot fee when rays of light do not enter the eye.

But it is time to profit by the many valuable obſervations we are now in poſſeſſion of. A liſt of ſucceſſive eminent aſtronomers may be named, from Gallileo down to the preſent time, who have furniſhed us with materials for examination.

In ſupporting the ideas I ſhall propoſe in this paper, with regard to the phyſical conſtruction of the ſun, I have availed myſelf of the labours of all theſe aſtronomers, but have been induced thereto only by my own actual obſervation of the ſolar phenomena; which, beſides verifying thoſe particulars that had been already obſerved, gave me ſuch views of the ſolar regions as led to the foundation of a very rational ſyſtem. For, having the advantage of former obſervations, my lateſt reviews of the body of the ſun were immediately directed to the moſt eſſential points; and the work was by this means facilitated, and contracted into a pretty narrow compaſs.

The following is a ſhort extract of my obſervations on the ſun, to which I have joined the conſequences I now believe myſelf entitled to draw from them. When all the reaſonings on the ſeveral phenomena are put together, and a few additional arguments taken from analogy, which I ſhall alſo add, are properly conſidered, it will be found that a general concluſion may be made which ſeems to throw a conſiderable light on our preſent ſubject.

In the year 1779 there was a ſpot on the ſun which was large enough to be ſeen with the naked eye. By a view of it with a ſeven feet reflector, charged with a very high power, it appeared to be divided into two parts. The largeſt of the two, on the 19th of April, meaſured 1′ 8″,06 in diameter; which is equal in length to more than 31 thouſand miles. Both together muſt certainly have extended above 50 thouſand.

The idea of its being occaſioned by a volcanic exploſion violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus reſtore the ſun, in that place, to its former ſplendour, ought to be rejected on many accounts. To mention only one, the great extent of the ſpot is very unfavourable to ſuch a ſuppoſition. Indeed a much

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lefs violent and lefs pernicious caufe may be affigned to account for all the appearances of the fpot. When we fee a dark belt near the equator of the planet Jupiter, we do not recur to earthquakes and volcanoes for its origin. An atmofphere, with its natural changes, will explain fuch belts. Our fpot on the fun may be accounted for on the fame principles. The earth is furrounded by an atmofphere compofed of various elaftic fluids. The fun alfo has its atmofphere; and if fome of the fluids which enter into its compofition fhould be of a fhining brilliancy, in the manner that will be explained hereafter, while others are merely tranfparent, any temporary caufe which may remove the lucid fluid will permit us to fee the body of the fun through the tranfparent ones. If an obferver were placed on the moon, he would fee the folid body of the earth only in thofe places where the tranfparent fluids of our atmofphere would permit him. In others, the opaque vapours would reflect the light of the fun without permitting his view to penetrate to the furface of our globe. He would probably alfo find that our planet had occafionally fome fhining fluids in its atmofphere; as, not unlikely, fome of our northern lights might not efcape his notice, if they happened in the unenlightened part of the earth, and were feen by him in his long dark night. Nay, we have pretty good reafon to believe, that probably all the planets emit light in fome degree; for the illumination which remains on the moon in a total eclipfe cannot be entirely afcribed to the light which may reach it by the refraction of the earth's atmofphere. For inftance, in the eclipfe of the moon October 22, 1790, the rays of the fun refracted by the atmofphere of the earth towards the moon, admitting the mean horizontal refraction to be 30′ 50″,8, would meet in a focus 189 thoufand miles beyond the moon; fo that confequently there could be no illumination from rays refracted by our atmofphere. It is, however, not improbable, that about the polar regions of the earth there may be refraction enough to bring fome of the folar rays to a fhorter focus. The diftance of the moon at the time of the eclipfe would require a refraction of 54′ 6″, equal to its horizontal parallax at

at that time, to bring them to a focus so as to throw light on the moon.

The unenlightened part of the planet Venus has also been seen by different persons, and not having a satellite, those regions that are turned from the sun cannot possibly shine by a borrowed light; so that this faint illumination must denote some phosphoric quality of the atmosphere of Venus.

In the instance of our large spot on the sun, I conclude from appearances that I viewed the real body of the sun itself, of which we rarely see more than its shining atmosphere.

In the year 1783 I observed a fine large spot, and followed it up to the edge of the sun's limb. Here I took notice that the spot was plainly depressed below the surface of the sun; and that it had very broad shelving sides. I also suspected some part, at least, of the shelving sides to be elevated above the surface of the sun; and observed that, contrary to what usually happens, the margin of that side of the spot which was farthest from the limb was the broadest.

The luminous shelving side of a spot may be explained by a gentle and gradual removal of the shining fluid, which permits us to see the globe of the sun. As to the uncommon appearance of the broadest margin being on that side of the spot which was farthest from the limb when the spot came near the edge of it, we may surmise that the sun has inequalities on its surface, which may possibly be the cause of it. For, when mountainous countries are exposed, if it should chance that the highest part of the landscape are situated so as to be near that side of the margin, or penumbra of the spot, which is towards the limb, it may partly intercept our view of it when the spot is seen very obliquely. This would require elevations at least five or six hundred miles high; but considering the great attraction exerted by the sun upon bodies at its surface, and the slow revolution it has upon its axis, we may readily admit inequalities to that amount. From the centrifugal force at the sun's equator, and the weight of bodies at its surface, I compute that the power of throwing down a mountain by the exertion of the former, balanced by the superior

ſuperior force of keeping it in its place of the latter, is near $6\frac{1}{2}$ times leſs on the ſun than on our equatorial regions; and as an elevation ſimilar to one of three miles on the earth would not be leſs than 334 miles on the ſun, there can be no doubt but that a mountain much higher would ſtand very firmly. The little denſity of the ſolar body ſeems alſo to be in favour of the height of its mountains; for, *cæteris paribus*, denſe bodies will ſooner come to their level than rare ones. The difference in the vaniſhing of the ſhelving ſide, inſtead of explaining it by mountains, may alſo, and perhaps more ſatisfactorily, be accounted for from the real difference of the extent, the arrangement, the height, and the intenſity of the ſhining fluid, added to the occaſional changes that may happen in theſe particulars during the time in which the ſpot approaches to the edge of the diſk. However, by admitting large mountains on the face of the ſun, we ſhall account for the different opinions of two eminent aſtronomers; one of whom believed the ſpots depreſſed below the ſun, while the other believed them elevated above it. For it is not impoſſible that ſome of the ſolar mountains may be high enough occaſionally to project above the ſhining elaſtic fluid, when, by ſome agitation, or other cauſe, it is not of the uſual height: and this opinion is much ſtrengthened by the return of ſome remarkable ſpots which ſerved Caſſini to aſcertain the period of the ſun's rotation. A very high country, or chain of mountains, may oftener become viſible, by the removal of the obſtructing fluid, than the lower regions, on account of its not being ſo deeply covered with it.

In the year 1791 I examined a large ſpot on the ſun, and found it evidently depreſſed below the level of the ſurface; about the dark part was a broad margin, or plane, of conſiderable extent, leſs bright than the ſun, and alſo lower than its ſurface. This plane ſeemed to riſe, with ſhelving ſides, up to the place where it joined the level of the ſurface.

In confirmation of theſe appearances, I carefully remarked that the diſk of the ſun was viſibly convex: and the reaſon of my attention to this particular, was my being already long acquainted with a certain optical deception, that takes place now and then when we view the moon; which is, that all the

the elevated ſpots on its ſurface will ſeem to be cavities, and all cavities will aſſume the ſhape of mountains. But then, at the ſame time, the moon, inſtead of having the convex appearance of a globe, will ſeem to be a large concave portion of an hollow ſphere. As ſoon as, by the force of imagination, you drive away the fallacious appearance of a concave moon, you reſtore the mountains to their protuberance, and ſink the cavities again below the level of the ſurface. Now, when I ſaw the ſpot lower than the ſhining matter of the ſun, and an extended plane, alſo depreſſed, with ſhelving ſides riſing up to the level, I alſo found that the ſun was convex, and appeared in its natural globular ſtate. Hence I conclude that there could be no deception in thoſe appearances.

How very ill would this obſervation agree with the ideas of ſolid bodies bobbing up and down in a fiery liquid? with the ſmoke of volcanoes, or ſcum upon an ocean? And how eaſily it is explained upon our foregoing theory. The removal of the ſhining atmoſphere, which permits us to ſee the ſun, muſt naturally be attended with a gradual diminution on its borders: an inſtance of a ſimilar kind we have daily before us, when through the opening of a cloud we ſee the ſky, which generally is attended by a ſurrounding hazineſs of ſome ſhort extent; and ſeldom tranſits, from a perfect clearneſs, at once to the greateſt obſcurity.

Aug. 26, 1792. I examined the ſun with ſeveral powers, from 90 to 500. It appears evidently that the black ſpots are the opaque ground, or body of the ſun; and that the luminous part is an atmoſphere, which, being interrupted or broken, gives us a tranſient glimpſe of the ſun itſelf. My 7-feet reflector, which is in high perfection, repreſents the ſpots, as it always uſed to do, much depreſſed below the ſurface of the luminous part.

Sept. 2, 1792. I ſaw two ſpots in the ſun with the naked eye. In the teleſcope I found they were cluſters of ſpots, with many ſcattered ones beſides. Every one of them was certainly below the ſurface of the luminous diſk.

Sept. 8, 1792. Having made a ſmall ſpeculum, merely brought to a perfect figure upon hones without poliſh, I found that, by ſtifling a great part of the ſolar rays, my ob-

ject-ſpeculum would bear a greater aperture; and thus enabled me to ſee with more comfort, and leſs danger. The ſurface of the ſun was unequal; many parts of it being elevated, and others depreſſed. This is here to be underſtood of the ſhining ſurface only, as the real body of the ſun can probably be ſeldom ſeen otherwiſe than in its black ſpots.

It may not be impoſſible, as light is a tranſparent fluid, that the ſun's real ſurface alſo may now and then be perceived; as we ſee the ſhape of the wick of a candle through its flame, or the contents of a furnace in the midſt of the brighteſt glare of it: but this, I ſhould ſuppoſe, will only happen where the lucid matter of the ſun is not very accumulated.

Sept. 9, 1792. I found one of the dark ſpots in the ſun drawn pretty near the preceding edge. In its neighbourhood I ſaw a great number of elevated bright places, making various figures: I ſhall call them *faculæ*, with Hevelius; but without aſſigning to this term any other meaning than what it will hereafter appear ought to be given to it. I ſee theſe faculæ extended, on the preceding ſide, over about 1-6th part of the ſun; but ſo far from reſembling torches, they appear to me like the ſhrivelled elevations upon a dried apple, extended in length, and moſt of them are joined together, making waves, or waving lines.

By ſome good views in the afternoon, I find that the reſt of the ſurface of the ſun does not contain any faculæ, except a few on the following, and equatorial part of the ſun. Towards the north and ſouth I ſee no faculæ: there is all over the ſun a great unevenneſs in the ſurface, which has the appearance of a mixture of ſmall points of an unequal light; but they are evidently an unevenneſs or roughneſs of high and low parts.

Sept. 11, 1792. The faculæ, in the preceding part of the ſun, are much gone out of the diſk, and thoſe in the following are come on. A dark ſpot alſo is come on with them.

Sept. 13, 1792. There are a great number of faculæ on the equatorial part of the ſun, towards the preceding and following parts. I cannot ſee any towards the poles; but a roughneſs is viſible every where.

 Sept. 16,

Sept. 16, 1792. The sun contains many large faculæ on the following side of its equator, and also several on the preceding side. I perceive none about the poles. They seem generally to accompany the spots; and probably, as the faculæ certainly are elevations, a great number of them may occasion neighbouring depressions, that is to say, dark spots.

The faculæ being elevations, very satisfactorily explains the reason why they disappear towards the middle of the sun, and re-appear on the other margin; for, about the place where we lose them, they begin to be edge-ways to our view; and if between the faculæ should lie dark spots, they will most frequently break out in the middle of the sun, because they are no longer covered by the side-views of these faculæ.

Sept. 22, 1792. There are not many faculæ in the sun, and but few spots; the whole disk, however, is very much marked with roughness, like an orange. Some of the lowest parts of the inequalites are blackish.

Sept. 23, 1792. The following side of the sun contains many faculæ near the limb. They take up an arch of about 30 degrees. There are likewise some on the preceding side. The north and south is rough as usual, but differently disposed. The faculæ are ridges of elevations above the rough surface.

Feb. 23, 1794. By an experiment I have just now tried, I find it confirmed that the sun cannot be so distinctly viewed with a small aperture and faint darkening glasses, as with a large aperture and stronger ones; this latter is the method I always use.

One of the black spots on the preceding margin, which was greatly below the surface of the sun, had, next to it, a protuberant lump of shining matter, a little brighter than the rest of the sun.

About all the spots, the shining matter seems to have been disturbed; and is uneven, lumpy, and zig-zagged in an irregular manner.

I call the spots black, not that they are entirely so, but merely to distinguish them; for there is not one of them to-day, which is not partly, or entirely, covered over with whitish and unequally bright nebulosity, or cloudiness. This, in

in many of them, comes near to an extinction of the ſpot; and in others, ſeems to bring on a ſubdiviſion.

Sept. 28, 1794. There is a dark ſpot in the ſun on the following ſide. It is certainly depreſſed below the ſhining atmoſphere, and has ſhelving ſides of ſhining matter, which riſe up higher than the general ſurface, and are brighteſt at the top. The preceding ſhelving ſide is rendered almoſt inviſible by the overhanging of the preceding elevations, while the following is very well expoſed; the ſpot being apparently ſuch in figure as denotes a circular form viewed in an oblique direction.

Near the following margin are many bright elevations, cloſe to viſible depreſſions. The depreſſed parts are leſs bright than the common ſurface.

The penumbra, as it is called, about this ſpot, is a conſiderable plane, of leſs brightneſs than the common ſurface, and ſeems to be as much depreſſed below that ſurface as the ſpot is below the plane.

Hence, if the brightneſs of the ſun is occaſioned by the lucid atmoſphere, the intenſity of the brightneſs muſt be leſs where it is depreſſed; for light, being tranſparent, muſt be the more intenſe the more it is deep.

Oct. 12, 1794. The whole ſurface of the ſun is diverſified by inequality in the elevation of the ſhining atmoſphere. The loweſt parts are every where darkeſt; and every little pit has the appearance of a more or leſs dark ſpot.

A dark ſpot, which is on the preceding ſide, is ſurrounded by very great inequalities in the elevation of the lucid atmoſphere; and its depreſſion below the ſame is bounded by an immediate riſing of very bright light.

Oct. 13, 1794. The ſpot in the ſun I obſerved yeſterday is drawn ſo near the margin, that the elevated ſide of the following part of it hides all the black ground, and ſtill leaves the cavity viſible, ſo that the depreſſion of the black ſpots, and the elevation of the faculæ, are equally evident.

[To be continued.]

III. *On the Production of Cast Iron, and the Operations of the Blast-Furnace. By Mr.* DAVID MUSHET, *of the Clyde Iron-Works. Communicated by the Author.*

IT was my wish, in the papers preceding this communication, to convey a clear and competent idea of the nature of iron as a metal; as also of ores, and iron-stones in general. I have endeavoured to explain, upon principles grounded on experiment, the chief agents of change in the smelting operation, so far as they affect the quality of the materials prepared. I have aimed at perspicuity rather than minuteness, which often becomes tedious; and although the facts may not be conveyed in a style so popular as communications, which profess a conveyance of practical truths, generally are, yet, I trust, this will form no real bar to their utility. When facts are to be learned, and principles satisfactorily explained, it is surely best to begin by the examination of simple causes; and tracing their agency according to their quantities, relative proportions, and affinities. These facts will undoubtedly, in the end, be more radically understood by using such phrases, or signs, as denote in each a respective quality affixed in consequence of a knowledge of its properties and effects. In short, if the language and reasoning of our manufactories is ever to become scientific; if philosophy and chemistry are ever to become of general utility in the perfection of any branch; then those truths which constitute the foundation of all science are not to be rejected, though clothed in a dress which at first sight appears discordant to our habits, or burdensome to our memory.

The operations I am about to describe have never as yet received any explanation consonant to true philosophy or chemical facts; yet there are few which present a more beautiful chain of affinities, decomposition, and recombination, than the manufacture of iron in all its various stages. An extensive foundry is a laboratory fraught with phenomena of the most interesting nature in chemistry and natural philosophy: are we not then justly surprised to find that prejudice

still

ftill reigns there; and that the curious manipulations of thefe regions are ftill fhrouded with error and mifconception; as if their dingy ftructure forbade the entrance of genius, or configned her laborious unlettered fons to an endlefs ftretch of mental obfcurity?

The plate of the blaft-furnace, given in the preceding Number, having a full defcription appropriated to it, I fhall proceed to detail the train of preparation neceffary before the furnace is brought to produce good melting iron.

The furnace being finifhed, the bottom and fides of it, for two feet up the fquare funnel, receive a lining of common bricks upon edge, to prevent the ftone from fhivering or mouldering when the fire comes in contact with it. On the front of the furnace is erected a temporary fire-place, about four feet long, into the bottom of which are laid correfponding bars. The fide-walls are made fo high as to reach the under-furface of the tymp-ftone; excepting a fmall fpace, which afterwards receives an iron plate of $1\frac{1}{2}$ inches thicknefs, by way of a cover: this alfo preferves the tymp-ftone from any injury it might fuftain by being in contact with the flame. A fire is now kindled upon the bars, and is fed occafionally with fmall coals. As the whole cavity of the furnace ferves as a chimney for this fire, the draught in confequence is violent, and the body of heat carried up is very confiderable. In the courfe of three weeks the furnace will thus become entirely free from damp, and fit for the reception of the materials: when this is judged proper the fire-place is removed, but the interior bricks are allowed to remain till the operation of blowing commences. Some loofe fuel is then thrown upon the bottom of the furnace, and a few bafkets of cokes are introduced; thefe are allowed to become thoroughly ignited before more are added. In this manner the furnace is gradually filled; fometimes entirely full, and at other times 5-8ths or 3-4ths full. The number of bafkets full depend entirely upon the fize of the furnace: that in the plate will contain 900 bafkets. If the coal is fplint, the weight of each bafket-full will be nearly 11 lb. $\times$ 900 = 99,000 lb. cokes. As this quality of cokes is made with a lofs of nearly 50 *per cent.* the original weight in raw

4 coals

coals will be equal to 198,000 lb. When we reflect that this vast body of ignited matter is replaced every third day, when the furnace is properly at work, a notion may be formed of the immense quantity of materials requisite, as also the consequent industry exerted to supply one or more furnaces for the space of one year.

When the furnace is sufficiently heated throughout, specific quantities of cokes, iron-stone, and blast-furnace cinders are added: these are called charges. The cokes are commonly filled in baskets, which, at all the various iron-works, are nearly of a size. The weight of a basket, however, depends entirely upon the nature and quality of the coal, being from 70 to 112 lb. each *. The iron-stone is filled into boxes, which, when moderately heaped, contain 56 lb. of torrefied iron-stone; they often exceed this when the stone has been severely roasted. The first charges which a furnace receives, contain but a small proportion of iron-stone to the weight of cokes: this is afterwards increased to a full burden, which is commonly 4 baskets cokes, 320 lb.; 2 boxes iron-stone, 112 lb.; 1 box blast-furnace cinders, 60 or 70 lb. †. At new works, where these cinders cannot be obtained, a similar quantity of limestone is used.

The descent of the charge, or burden, is facilitated by opening the furnace below two or three times a-day, throwing out the cold cinders, and admitting, for an hour at a time, a body of fresh air. This operation is repeated till the approach of the iron-stone and cinder, which is always announced by a partial fusion, and the dropping of lava through the iron bars, introduced to support the incumbent materials while those on the bottom are carried away. The filling above is regularly continued, and when the furnace at the

* This same variety in the coal renders it almost impossible, under one description, to give a just idea of the proportions used at various blast-furnaces: to avoid being too diffuse, I shall confine my description connected with a coal of a medium quality, or a mixture of splint and free-coal, a basket of which will weigh from 78 lb. to 84 lb.

† A preference at first is always given to blast-furnace cinders in place of lime; being already vitrified, they are of much easier fusion, and tend to preserve the surface of the hearth by glazing it over with a black vitrid crust.

top

top has acquired a confiderable degree of heat, it is then judged time to introduce the blaft; the preparations neceffary for which are the following:—

The dam-ftone is laid in its place firmly imbedded in fire-clay; the dam-plate is again imbedded on this with the fame cement, and is fubject to the fame inclination. On the top of this plate is a flight depreffion, of a curved form, towards that fide fartheft diftant from the blaft, for the purpofe of concentrating the fcoria, and allowing it to flow off in a connected ftream, as it tends to furmount the level of the dam. From this notch to the level of the floor a declivity of brick-work is erected, down which the fcoria of the furnace flows in large quantities. The opening betwixt the dam and fide-walls of the furnace, called the *fauld*, is then built up with fand, the loofe bricks are removed, and the furnace bottom is covered with powdered lime or charcoal-duft. The ignited cokes are now allowed to fall down, and are brought forward with iron bars nearly to a level with the dam. The fpace between the furface of the cokes and the bottom of the tymp-plate is next rammed hard with ftrong binding fand; and thefe cokes, which are expofed on the outfide, are covered with coke-duft. Thefe precautions being taken, the tuyere-hole is then opened and lined with a foft mixture of fire-clay and loam: the blaft is commonly introduced into the furnace at firft with a fmall difcharging-pipe, which is afterwards increafed as occafion may require. In two hours after blowing, a confiderable quantity of lava will be accumulated; iron bars are then introduced, and perforations made in the compreffed matter at the bottom of the furnace; the lava is admitted to all parts of the hearth, and foon thoroughly heats and glazes the furfaces of the fire-ftone. Shortly after this it rifes to a level with the notch in the dam-plate, and by its own accumulation, together with the forcible action of the blaft, it flows over. Its colour is at firft black; its fracture denfe, and very ponderous; the form it affumes in running off is flat and branched, fometimes in long ftreams, and at other times lefs extenfive. If the preparation has been well conducted, the colour of the cinder will foon change to white; and the metal, which in the ftate of an oxyde formerly

coloured

coloured it, will be left in a disengaged state in the furnace. When the metal has risen nearly to a level with the dam, it is then let out by cutting away the hardened loam of the fauld, and conveyed by a channel, made in sand, to its proper destination; the principal channel, or runner, is called the *sow*, the lateral moulds are called the *pigs*.

In six days after the commencement of blowing, the furnace ought to have *wrought herself clear*, and have acquired capacity sufficient to contain from 5000 to 7000 weight of iron. The quality ought also to be richly carbonated, so as to be of value and estimation in the pig-market. At this period, with a quality of coal as formerly mentioned, the charge will have increased to the following proportions:—5 baskets cokes, 400lb.; 6 boxes iron-stone, 336lb.; 1 box limestone, 100lb.

An analysis of the smelting operation, and the tendency which the individual agents have to produce change in the quality and quantity of the iron, come next under consideration. Let us, however, first notice the characteristic features exhibited by the different kinds of iron while in fusion, whereby the quality of the metal may be justly defined.

When fine (No. 1.) or supercarbonated crude iron is run from the furnace, the stream of metal, as it issues from the fauld, throws off an infinite number of brilliant sparkles of carbon. The surface is covered with a fluid pellicle of carburet of iron, which, as it flows, rears itself up in the most delicate folds: at first the fluid metal appears like a dense, ponderous stream, but, as the collateral moulds become filled, it exhibits a general rapid motion from the surface of the pigs to the centre of many points; millions of the finest undulations move upon each mould, displaying the greatest nicety and rapidity of movement, conjoined with an uncommonly beautiful variegation of colour, which language is inadequate justly to describe. Such metal, in quantity, will remain fluid for twenty minutes after it is run from the furnace, and when cold will have its surface covered with the beautiful carburet of iron, already mentioned, of an uncommonly rich and brilliant appearance. When the surface of the metal is not carbureted, it is smooth like forged iron, and always

always convex. In this ſtate iron is too rich for melting without the addition of coarſe metal, and is unfit to be uſed in a cupola furnace for making fine caſtings, where thinneſs and a good ſkin are requiſite.

No. 4, or oxygenated crude iron, when iſſuing from the blaſt-furnace, throws off from all parts of the fluid ſurface a vaſt number of metallic ſparks: they ariſe from a different cauſe than that exerted in the former inſtance. The extreme privation of carbon renders the metal ſubject to the combination of oxygen ſo ſoon as it comes into contact with atmoſpheric air. This truth is evidently manifeſted by the ejection of ſmall ſpherules of iron from all parts of the ſurface: the deflagration does not, however, take place till the globule has been thrown two or three feet up in the air; it then inflames and ſeparates, with a ſlight hiſſing exploſion, into a great many minute particles of brilliant fire. When theſe are collected they prove to be a true oxyde of iron, but ſo much ſaturated with oxygen as to poſſeſs no magnetic obedience. The ſurface of oxygenated iron, when running, is covered with waving flakes of an obſcure ſmoky flame, accompanied with a hiſſing noiſe; forming a wonderful contraſt with the fine rich covering of plumbago in the other ſtate of the metal, occaſionally parting and exhibiting the iron in a ſtate of the greateſt apparent purity, agitated in numberleſs minute fibres, from the abundance of the carbon united with the metal.

When iron thus highly oxygenated comes to reſt, ſmall ſpecks of oxyde begin to appear floating upon the ſurface: theſe increaſe in ſize; and when the metal has become ſolid, the upper ſurface is found entirely covered with a ſcale of blue oxyde of various thickneſſes, dependent upon the ſtage of oxygenation or extreme privation of carbon. This oxyde, in common, contains about 15 *per cent.* of oxygen, and is very obedient to the magnet. In place of a dark blue ſmooth ſurface, convex and richly carbonated, the metal will exhibit a deep, rough, concave face, which, when the oxyde is removed, preſents a great number of deep pits. This iron in fuſion ſtands leſs convex than carbonated iron, merely becauſe it is leſs ſuſceptible of a ſtate of extreme diviſion; and

 indeed

indeed it seems a principle in all metallic fluids, that they are convex in proportion to the quantity of carbon with which they are saturated. This iron flows dead and ponderous, and rarely parts in shades but at the distance of some inches from each other.

This is a slight sketch of the appearance of the two extreme qualities of crude, or pig-iron, when in a state of fusion. According to the division formerly made, there still remains two intermediate stages of quality to be described: these are, carbonated and carbo-oxygenated iron; that is, No. 2 and 3 of the manufacturers. Carbonated iron exhibits, like No. 1, a beautiful appearance in the runner and pig. The breakings of the fluid, in general, are less fine; the agitation less delicate; though the division of the fluid is equal, if not beyond that of the other. When the internal ebullition of the metal is greatest, the undulating shades are smallest and most numerous: sometimes they assume the shape of small segments; sometimes fibrated groups; and at other times minute circles, of a mellower colour than the ground of the fluid. The surface of this metal, exposed to external air, when cooling is generally slightly convex, and full of punctures: these, in iron of a weak and fusible nature, are commonly small in the diameter, and of no great depth. In strong metal, the punctures are much wider and deeper. This criterion, however, is not infallible, when pig-iron of different works is taken collectively. At each individual work, however, that iron will be strongest whose honeycombs are largest and deepest.

Carbo-oxygenated, or No. 3, pig-iron, runs smoothly, without any great degree of ebullition or disengagement of metallic sparks. The partings upon its surface are longer, and at greater distances from each other than in the former varieties; the shape they assume is either elliptical, circular, or curved. In cooling, this metal acquires a considerable portion of oxyde; the surface is neither markedly convex nor concave; the punctures are less, and frequently vanish altogether. Their absence, however, is no token of a smooth face succeeding: in qualities of crude iron oxygenated beyond this, I have already mentioned that a concave surface

is

is the conſequence of the extreme abſence of carbon; and that, in proportion as this principle is abſent, the ſurface of the iron acquires roughneſs and aſperity.

It may perhaps be proper here to mention, once for all, that although, for convenience, the manufacturer has, from a juſt eſtimation of the value of the metal in a ſubſequent manufacture, affixed certain numbers for determinate qualities of iron, yet it is difficult to ſay at what degree of ſaturation of carbon each reſpective term commences: ſuffice it then to ſay, that the two alterative principles, oxygen and carbon, form two diſtinct claſſes, that in which oxygen predominates, and that in which carbon predominates; the latter comprehends No. 1 and 2 of the manufacturers, the former includes oxygenated, white and mottled; and the equaliſation of theſe mixtures form, as has already been noticed, the variety of carbo-oxygenated crude iron.

I ſhall now obſerve ſome things relative to the various faces which crude iron aſſumes. No. 1 and 2, with their intermediate qualities, poſſeſs ſurfaces more or leſs convex, and frequently with thin bliſters: this we attribute to the preſence of carbon, which being plentifully interſperſed betwixt and throughout the particles of the metal, the tendency which the iron has to ſhrink in cooling is entirely done away; it tends to diſtend the aggregate of the maſs, and to give a round face, by gradually elevating the central parts of the ſurface, which are always laſt to loſe their fluidity.

Again, that quality of iron known by the name of No. 3, or carbo-oxygenated, is moſt commonly found with a flat ſurface. If we ſtill farther trace the appearance of the ſurface of pig-iron, when run from the furnace, we ſhall find No. 4, either with a white or mottled fracture, poſſeſſed of concave faces rough and deeply pitted. Beyond this it may be imagined that every degree of further oxygenation would be productive of a ſurface deeper in the curve, and rougher, with additional aſperities. The contrary is the caſe: when crude iron is ſo far debaſed as to be run from the furnace in clotted lumps highly oxygenated, the ſurface of the pigs is found to be more convex than that of No. 1 iron; but then

the fracture of such metal presents an impure mass covered on both faces with a mixture of oxydated iron, of a blueish colour, nearly metallic. In short, this quality of iron is incapable of receiving such a degree of fluidity as to enable us to judge whether the convexity of its surface is peculiar to its state, or is owing to its want of division as a fluid, whereby the gradual consolidation of the metal is prevented.

These features sufficiently distinguish betwixt the various qualities of crude iron after they are obtained from the blast-furnace: there are, however, criterions not less infallible, whereby we can prejudge the quality of the metal many hours before it is run from the furnace. These are the colour and form of the scoria, the colour of the vitrid crust upon the working bars, and the quantity of carburet which is attached to it. The variety of colour and form in the cinder almost universally indicate the quality of the metal on the hearth. Hence, from a long course of experience, have arisen the following denominations: "Cinder of sulphury iron;" "Cinder of No. 1, No. 2, and No. 3;" and "Cinder of ballast iron." Although at different works, from local circumstances, the same kind of scoria may not indicate precisely the same quality of iron, yet the difference is so small that the following description of the various cinders may convey a very just idea of their general appearance.

When the scoria is of a whitish colour and short form, branching from the notch of the dam, and emitting from its stream beautiful sparks of ignited carbon, resembling those ejected from a crucible of cast steel in fusion, exposed to external air, or to the combustion of fine steel filings in a white flame; if, when issuing from the orifice of the furnace, it is of the purest white colour, possessing no tenacity, but in a state of the greatest fluid division, and, when cold, resembles a mass of heavy torrefied spar, void of the smallest vitrid appearance, hard and durable, it is then certain that the furnace contains *sulphury iron*, i. e. super-carbonated iron. At blast-furnaces, where a great quantity of air is thrown in *per* minute, super-carbonated crude iron will be obtained with a cinder of a longer form, with a rough flinty fracture towards the outside of the column.

That

That cinder which indicates the presence of carbonated iron in the hearth of the furnace, forms itself into circular compact streams, which become consolidated and inserted into each other; these are in length from three to nine feet. Their colour, when the iron approaches the first quality, is a beautiful variegation of white and blue enamel, forming a wild profusion of the elements of every known figure; the blues are lighter or darker according to the quantity of the metal and the action of the external air while cooling. When the quality of the pig-iron is sparingly carbonated, the blue colour is less vivid, less delicate; and the external surface rougher, and more sullied with a mixture of colour. The same scoria, when fused in vessels which are allowed to cool gradually, parts with all its variety of light and shade, and becomes of a yellowish colour, sometimes nearly white when the quantity of incorporated metal has been small.

The cinder which is emitted from the blast-furnace when carbo-oxygenated (or No. 3,) iron is produced, assumes a long zig-zag form. The stream is slightly convex in the middle; broad, flat, and obliquely furrowed towards the edges. The end of the stream frequently rears itself into narrow tapered cones, to the height of six or eight inches: these are generally hollow in the centre, and are easily demolished, owing to their excessive brittleness. The colour of this lava is very various; for the most part it is pale yellow mixed with green. Its tenacity is so great, that if, while fluid, a small iron hook is inserted into it at a certain degree of heat, and then drawn from it with a quick but steady motion, 20 to 30 yards of fine glass thread may be formed with ease. If the colours are vivid and variegated, the thread will possess, upon a minute scale, all the various tints of colouring which is found in the columnar mass. When by accident a quantity of this lava runs back upon the discharging-pipe, it is upon the return of the blast impelled with such velocity as to be blown into minute delicate fibres, smaller than the most ductile wire; at first they float upon the air like wool, and when at rest very much resemble that substance.

The presence of oxygenated crude iron (No. 4,) on the furnace-

furnace-hearth, is indicated by the lava resolving itself into long streams, sometimes branched, sometimes columnar, extending from the notch to the lowest part of the declivity; here it commonly forms large, flat, hollow cakes, or inclines to form conical figures: these are, however, seldom perfect; for the quantity of fluid lava, conveyed through the centre of the column, accumulates faster than the external sides of the cone are consolidated; and thus, when the structure is only half finished, the small crater vomits forth its superabundant lava, and is demolished. The current of such lava falls heavily from the dam as if surcharged with metal, and emits dark red sparks resembling the agitation of straw embers. Its colour is still more varied than the former descriptions of scoriæ, and is found changing its hues through a great variety of greens shaded with browns. Another variety of scoria, which indicates the same quality of iron, assumes a similar form; but has a black ground colour mixed with browns, or is entirely black. When the latter colour prevails, the texture of the cinder becomes porous; the quantity of iron left is now very considerable, and such as will be easily extracted in the assay-furnace with proper fluxes. In cases of total derangement in the furnace, the scoria will still retain this black colour, although the quantity of metal may amount to 25 *per cent.*; the fracture, however, becomes dense, and its specific gravity increases in proportion to the quantity of metal it holds incorporated.

The next source of information, as to the quality of the iron in the furnace, is to be got from the colour of the scoria upon the working bars, which are from time to time inserted to keep the furnace free from lumps, and to bring forward the scoria. When super-carbonated crude iron is in the hearth, the vitrid crust upon the bars will be of a black colour and smooth surface, fully covered with large and brilliant plates of plumbago.

As the quality of the metal approaches to No. 2 (carbonated), the carburet upon the scoria decreases both in point of quantity and size.

When carbo-oxygenated iron (No. 3) is in the furnace, the working bars are always coated with a lighter co-

loured ſcoria than when the former varieties exiſt; a ſpeck of plumbago is now only found here and there, and that of the ſmalleſt ſize. When the quality of the metal is oxygenated (No. 4.), not only have the plates of carburet diſappeared, but alſo the coally colour on the external ſurface of the ſcoria; what now attaches to the bars, is nearly of the ſame nature and colour as the lava emitted at the notch of the dam.

Theſe criterions are infallible; for, as the fuſibility or carbonation of the metal is promoted in a direct ratio to the comparative quantity of the coally principle preſent in the furnace, ſo in the ſame proportion will the vitrid cruſt encircling the working bars exhibit the preſence of that principle in the furnace.

IV. *Agenda, or a Collection of Obſervations and Reſearches, the Reſults of which may ſerve as the Foundation for a Theory of the Earth. By* M. De Saussure.

[Continued from Page 29.]

CHAP. XXII.

Errors to be avoided in Obſervations reſpecting Geology.

1. THERE are ſome errors into which people may readily fall when they have not had long experience in any given kind of obſervation, and againſt which it is of importance to put beginners at leaſt on their guard.

2. One may be readily deceived in regard to the relative diſtances of remote objects. All the ſtars and planets appear to be at an equal diſtance from us. Diſtant mountains all appear to be in the ſame plain. Thus thoſe which are ſituated very far behind the reſt, ſeem to form one body with them; ſo that people believe they ſee continued and uninterrupted chains when there are really none, and where the mountains, on the contrary, are inſulated.

The abſolute diſtance of objects, even when not very remote, is equally difficult to be aſcertained on high mountains,

tains, where the tranſparency of the air, and the abſence of vapours, deſtroy the aërial perſpective. I have often imagined that I had only two or three hundred ſteps to make in order to reach a ſummit, the diſtance of which from me was more than a league in a ſtraight line.

3. There are a great many errors in regard to ſtrata. Their great thickneſs may make one believe that there are none where they really exiſt. In the like manner, if the vertical ſtrata, or thoſe only very much inclined, preſent their planes to the eye of the obſerver, he will think he ſees ſhapeleſs and indiviſible maſſes; while, if their ſections were ſeen, their diviſions would readily be diſtinguiſhed. A mountain then muſt be ſeen under aſpects that interſect each other at right angles before we can pronounce that it is not divided by ſtrata.

4. At other times accidental fiſſures, but produced however by a cauſe which is common to them, exhibit the appearance of ſtrata when there are none; or when, if there are, their ſituation is very different from that of thoſe ſtrata. It is the internal tiſſue of the ſtone only which in many caſes can determine whether the diviſions obſerved are the interſtices between ſtrata or mere fiſſures; becauſe the ſtrata are conſtantly parallel to the internal laminæ, or ſchiſtous texture of the ſtone. Cryſtals, the lamellated texture of which may ſometimes be confounded with a ſchiſtous texture, may afford an exception to this rule, by preſenting laminæ perpendicular to the planes of the ſtrata; but it is not difficult to diſtinguiſh them.

5. One may alſo form an erroneous opinion reſpecting the direction of a mountain, or of its ſtrata, when the eye is not ſituated in their prolongation, or at leaſt near it.

6. The apparent ſituation of the ſtrata may alſo lead into an error. They appear horizontal even when they are very much inclined, and when they are not ſeen but in a ſection formed by a plane parallel to the common ſection of their planes with the horizon. It is impoſſible to judge of their inclination, and to meaſure it with certainty, but on a ſection perpendicular to the common ſection, which I have juſt mentioned.

6. A. The

6. A. The greateſt error, however, is that which may be committed in regard to the ſuper-poſition of ſtrata. I have often ſeen novices in the ſtudy of mountains believe that one ſtratum repoſed on another; one of granite, for example, on one of ſlate; becauſe they found ſlate at the bottom of the mountain, and granite at the top; while the ſlate was only laid againſt the baſe of the mountain, and the granite, on the other hand, was ſunk in the earth far below the ſlate. We muſt not then ſay, that a ſtratum is ſituated below another, but when we really ſee it extending itſelf below it.

7. And even when we diſtinctly ſee a rock placed above another, we muſt examine whether that which is uppermoſt does not occupy that ſituation accidentally; whether it has not ſlipped, or rolled down, from a more elevated mountain; and, in the laſt place, though they may be cloſely connected, one muſt examine whether their preſent ſituation is really the ſame in which they were formed, and whether they have not been reverſed, and united accidentally in a ſituation contrary to that of their original formation.

8. One is frequently deceived, alſo, in regard to the nature of ſtones and of mountains. Though a well-accuſtomed eye may often judge at ſome, and even a conſiderable diſtance, of the kind of ſtone of which a mountain is compoſed, ſuch judgment is however often erroneous: mountains of granite, or *gneiſs*, tender and deſtructible, often aſſume, at a diſtance, the round form of ſecondary mountains; ſometimes, alſo, mountains of calcareous ſtone, hard of their kind, and in ſtrata either vertical or very much inclined, preſent the bold forms, the peaks, and ſharp-angled indentations of the granite ſummits.

9. People are often deceived even on a near view. A ſtone may have a foreign covering of mica, for example, while the interior part is of a very different nature.

10. Effervеſcence with the nitrous acid is commonly conſidered as a certain character of calcareous ſtone; but this character may be deceptive, ſince barytes and magneſia effervеſce alſo *: and we muſt not conſider it enough to touch

* And, on the other hand, there are calcareous ſtones which do not effervеſce.

a ftone with the nitrous acid, or to let fall a drop of the acid on its furface, fince the abforbing earth, whatever it is, may be only diffeminated between argillaceous or filiceous particles. We muft therefore immerfe a fragment of the ftone in a quantity of the acid fufficient to diffolve it entirely, if it be wholly foluble, and obferve whether there remains any refiduum that withftands folution.

11. The action of the air and of meteors often gives foffils appearances abfolutely different from thofe which they had before they were fubjected to it. We muft not then be fatisfied with a fuperficial examination: we muft found the rocks to the quick where the action of meteoric agents has not penetrated.

12. People are often deceived, alfo, in confidering compound ftones as fimple ftones, when the compofition of them does not manifeft itfelf on the firft view, either on account of the fmallnefs of their compofing parts, or becaufe fome of thefe parts are each inclofed feparately in a covering which conceals the interior of them. One may guard againft this error by obferving the foffil in the fun with ftrong magnifying glaffes, after having moiftened its furface with water or the nitrous acid, and ftill better by expofing it gradually to the flame of the blow-pipe.

13. People are often deceived in regard to cryftallifation, either in the true form of the cryftals, or, above all, in taking for real cryftals parafite cryftals, or fuch as have been formed in the moulds made by cryftals of another kind. Thus we fee cryftals of quartz, petro-filex, and jafper, formed in the moulds made by calcareous cryftals, and which have affumed the form of the latter.

14. In regard to errors occafioned by ignorance of the diftinctive characters of foffils, and of the names proper for them, the only means of avoiding fuch errors is to ftudy with care good authors; and, above all, collections formed, or at leaft arranged and titled, by able mineralogifts.

15. But when the flighteft doubt is entertained in regard to the denomination which ought to be given to any foffil, an exact defcription muft be made either of its external characters or its moft ftriking phyfical properties, fuch as weight and

and ſolubility *. If this deſcription is well drawn up, the error reſpecting the name may be rectified, and the obſervation will not be loſt, as it would be were there any reaſon to ſuſpect the juſtneſs of the denomination, and no means of correcting it by a deſcription †.

16. When the characters of a foſſil give it ſuch a likeneſs to another that it is found near the limits which ſeparate the genera or ſpecies of theſe two foſſils, we muſt follow the example of Werner and his diſciples, by marking that this foſſil is intermediary, or forms a tranſition from the one ſpecies to the other. For if we ſhould aſcribe it excluſively to the genus A, without noting the characters which bring it near to the genus B, another obſerver, on ſeeing the ſame foſſil, might refer it to the genus B, and no one could know which of them was deceived.

17. People are often deceived alſo by mixing opinion with obſervation, and giving the former for the latter; as when people aſſert, that they have ſeen veſtiges of extinguiſhed volcanoes, becauſe they have ſeen black or porous ſtones, or ſtones of a priſmatic form, without deigning to deſcribe them with care, but by qualifying them merely as *lava* or *baſaltes*.

18. In the laſt place, a very frequent ſource of error is, too great a confidence in the fidelity of one's memory, or in the juſtneſs of one's firſt obſervations. Theſe two kinds of confidence go often hand in hand; and people cannot guard againſt the errors, which are the conſequence of them, but by noting down, on the ſpot, all obſervations to which any importance is attached, eſpecially if they are a little complex, and carry away ſpecimens, with their characters carefully marked upon them, of the objects that are the ſubject of

* Hardneſs, refrangibility, electricity, &c. H.

† A perſon now dead, who in his time was conſidered as a mineralogiſt, wrote to me that he had found marine ſhells in granite. I begged him to give me an exact deſcription of the ſtone which he called *granite*. He did ſo; but I perceived that the ſtone was a free-ſtone or ſand-ſtone, and the ſpecimens he afterwards ſent me proved that I was not deceived. We may here recall M. Bergman's pyrites of Ætna. The errors of this kind, ariſing from falſe denominations, are innumerable; for an exact knowledge of mineral differences is more difficult to be obtained, and more rare, than is generally imagined.—Note of the AUTHOR.

theſe obſervations; for it is not ſpecimens of rare objects merely that ſhould be collected. The end, indeed, of the geological obſerver is, not to form a cabinet of curioſities, but he muſt carry away fragments of things apparently the moſt common, when an exact determination of their nature may be intereſting to theory. People may thus employ, with advantage, the means of confirming or rectifying their firſt obſervations, and of making profound reſearches and compariſons impoſſible to be made on the ſpot *.

[To be continued.]

V. *Obſervations on Animal Electricity, and particularly that called Spontaneous. By* J. J. Hemmer.

[Concluded from Page 7.]

In a letter, dated June 21, 1787, which I received from M. De Sauſſure, he confeſſes that he had not made any farther experiments on animal electricity; and that he did not know whether any had been made by others. As I was convinced, however, that a complete knowledge of that electricity which is produced in the human body by the friction of the clothes, as well as ſpontaneouſly, might be of great utility

* We think it our duty to ſubjoin here ſome advice to travellers in regard to the queſtions which they may aſk in the different towns.

Whence do they procure the materials proper for building; ſuch as lime, plaſter, tiles, ſlate, ſtones of different kinds, and ſand? Do they burn turf or coal; and where are they found? Where do they procure their potter's clay, fuller's earth, the clay uſed for refining ſugar, their whetſtones and millſtones? To obſerve with what the ſtreets are paved; of what ſtone the ſteps of ſtairs are formed; marks for boundaries, &c.; and to learn from what place they are brought. To aſcertain whether wells or the foundation of houſes are dug; and whether there are in the neighbourhood any ravines or precipices. Theſe queſtions will ſerve to facilitate the means of obſerving the nature of the ground, by pointing out the natural or artificial excavations that may exiſt in the neighbourhood, or which ought to be viſited. For the ſame reaſon it is proper to examine the ſhores of rivers. It will be of uſe alſo to take a general view of the country from the tops of towers and of the higheſt ſteeples. It will be of ſome importance alſo to enquire, in the country, whether the inhabitants make uſe of lime, marl, plaſter, coal, earth, or turf-aſhes, for manuring their land; and from what places theſe ſubſtances are procured. C.

utility both to the ſcience of electricity in general, and particularly to medical electricity, I reſolved to make reſearches on this ſubject by means of experiments. The method I employed was as follows:—In order that I might examine the electricity of my own body, I inſulated myſelf on a board which ſtood on glaſs feet. I then touched, for a determined time, (at firſt commonly half a minute, afterwards only a moment,) the plate of my condenſer, which I have deſcribed in another place*; I then applied the condenſer to a Cavallo's highly ſenſible electrometer, as improved by M. De Sauſſure, and, by means of a glaſs tube rubbed with a piece of woollen cloth, examined the kind of electricity when announced by the diverging threads. The electricity of the plate of the condenſer or electrometer, which correſponded with that of the body, I marked with + when poſitive, with — when negative, and with o when none was preſent. I made my experiments on the 21ſt of February 1786; and ſince that period have repeated them, not only on myſelf, but on other perſons, both male and female, of various ages and different conſtitutions, when they were in motion, or at reſt; dreſſed and undreſſed; when tired, and in good ſpirits; hot or cold; faſting or full; ſleeping or awake; at different temperatures of the weather and of the apartment, &c.

Reſults of the Experiments.

1. Animal electricity is common to all men; for, as I found it, in the courſe of my experiments, on thirty perſons of both ſexes and all ages, and of every habit of body, I can with juſtice conclude on its being general.

2. Animal electricity is different in different men, at the ſame place and at the ſame time; not only in ſtrength, but alſo in the kind; being in many weak, and others ſtrong; in ſome poſitive, and in ſome negative.

3. This difference of electricity is often obſerved in different perſons, when, beſides time and place, the other circumſtances appear to be all equal.

4. The ſtrength and kind of this electricity are often different;

* See *Gren's Journal der Phyſik*, Vol. II. Part II. p. 210.

ferent; not only in different men, but also in the same person. In 2422 experiments, in which I examined my own electricity, I found it 1252 times positive, 771 times negative, and 399 times 0. The electricity of my maid, in 94 experiments, was, on the other hand, 19 times positive, 33 times negative, and 42 times 0.

5. It often happens, that, during the experiment, the electricity changes; that is, from positive to negative, or from stronger positive to weaker, and at last becomes 0. It then passes from the last to weak negative, and gradually to stronger.

6. It happens also, not unfrequently, that the electricity, at the commencement of the experiment, is strongly positive or negative, but passes afterwards to the opposite kind with the like force, which continues for a considerable time.

7. Notwithstanding this wonderful and almost continual alternation, animal electricity appears to be naturally positive. As the electric matter is diffused throughout all nature, and adheres, in a fixed form, to no body, all the articles of our nourishment contain each their share of it. The electric fluid is disengaged, and diffused with the blood and juices through the body; from which it is conveyed off, after being accumulated in it, by the pores of the skin and other ducts, when there is no interruption in them. This is confirmed by experience; for when animal electricity is examined in a body exposed to no violent exertions, as when a person sits or reposes, and when there is consequently no great loss of heat, it is commonly positive. Thus the electricity of my body, which I examined while sitting at rest, when the natural heat of the body was not disturbed, appeared, in 332 experiments positive, in 14 negative, and in 10 was 0.

8. When animal electricity, therefore, is 0, or negative, the body must be in an unnatural state *(statu violento)*.

9. Cold changes natural or positive animal electricity into the opposite kind, or at least lessens it. In 60 experiments I made on myself after coming from a cold air, the temperature of which was at the freezing point or below it, my electricity was 38 times negative, 7 times 0, and 15 times positive, the last being not unfrequently weak. I had often

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an opportunity of obſerving the pleaſant phenomenon, that the negative electricity ariſing from cold, after pulling off a cold and putting on a warm coat, was ſpeedily converted into poſitive, or the degrees next to it. When the thermometer ſtood ſeveral degrees above the freezing point, and a ſomewhat cold wind prevailed, the latter was ſufficient to produce negative or 0 electricity, as I experienced four times. Of 13 times, while I ſat lightly clothed in a warm apartment, the electricity was 8 times 0, thrice −, and twice weakly +. Nay, when I clothed myſelf well, and remained any time at a cooler part of the room, ſuch as the open door or window, I found the electricity either negative or weakly poſitive. For producing this effect, it is in general ſufficient if a part only of the warm body be expoſed to cold. When the electricity was commonly +, while I ſat at perfect reſt, on my uſual chair, I oftentimes remarked that it was perceptibly negative if my feet were extremely cold, without being able to aſſign any other cauſe for this phenomenon. It is, however, fully proved by the following circumſtance: During 23 times that I waſhed my hands and face with cold water, the electricity was only 15 times perceptibly +, 8 times —, thrice 0, and 7 times weakly +; though a moment before, or after, it was, for the moſt part, ſtrongly +.

That cold impedes poſitive electricity, or makes it leſs and reduces it to 0, may be eaſily comprehended; becauſe cold cauſes every thing to contract, leſſens the pores of animal bodies, and, by theſe means, obſtructs the eſcape of the electric matter. That it, however, converts this electricity into negative, or gives to the external part of animal bodies the property of extracting from other bodies, when in contact with them, a part of their natural electricity, is not eaſy to be explained; for, in order to do ſo, they muſt themſelves have ſuſtained a loſs of their natural quantity of electricity. But when, by the contraction of the pores, they receive no more of the electric matter from the interior parts of the body, they muſt, in order that the equilibrium be kept up, not ſuſtain any loſs of it. This loſs is either real or apparent. The former can ſcarcely be affirmed, as there are not ſufficient grounds for ſuch an idea. For if it be ſaid that the electric

matter naturally contained in the external part of the body, is expelled by that accumulated more and more in the interior parts, on account of the cold, and which thereby acquires a ſtronger repulſive power, it is of no weight; ſince this power would prevent the plate of the condenſer from giving up any of its natural electricity to the finger when applied to it. There is more reaſon, therefore, to aſſert the latter, *viz.* that this want is only apparent; that is, that the electric fluid, which the external parts of the body naturally poſſeſs, is not expelled by the effects of cold, but fixed, or ſo faſt united with the parts in which it is contained, that it loſes all movement, and gives no indications of its preſence. This kind of union is not uncommon in nature: it takes place in regard to the particles of fire, which produce heat; when moiſt bodies evaporate, or when ſalt is mixed with ice.

10. Laſſitude impedes alſo the poſitive animal electricity, or changes it into the oppoſite. During 16 times that I walked backwards and forwards in my apartment, or was otherwiſe employed, the temperature of the weather being 10, 12, 14, and more degrees of heat, and no cold wind prevailing at the time, I found the electricity only once weakly +, five times 0, and ten times —. In 32 experiments, when ſtanding at reſt, the electricity was 30 times 0, and twice weakly +. In 27 experiments, while I ſat at reſt, it was always ſtrongly +; and in five experiments, while walking at a moderate pace, it was perceptibly +. I found the caſe to be the ſame on my ſervant and another young man.

I will not, however, ſay that the electricity, when the body is in a ſtate of reſt, always ceaſes, or is changed from poſitive. Many times the ſtrength of the body is greater than to allow of its being exhauſted by fatigue, as I have more than once experienced.

11. By rubbing a part of the body, poſitive electricity is not impeded. The rubbing may be performed with linen or woollen.

12. Sudden, ſpeedy, and violent motion can change every kind of animal electricity into the oppoſite. A luminous and beautiful appearance was occaſioned when I inclined and raiſed my body ſuddenly in turns, and threw upwards quickly

and

and with a certain degree of violence, fometimes my arms and fometimes my legs. Under thefe circumftances the electricity was thrice changed from — to +, and 16 times from 0, or weak +, to —. On my fervant, under the fame circumftances, it was eight times changed from + to —; and on my maid five times from — to +.

13. Every other motion of the body, not connected with fuch violence, or with uncommon agitation of the limbs, when it does not produce much fatigue, does not impede pofitive animal electricity.

14. Such motion is no impediment alfo to negative animal electricity.

15. After noon, or dinner-time, animal electricity is not greater than common.

16. The ufe of coffee makes no change in animal electricity.

17. Continual ftraining of the thoughts is not only favourable to pofitive animal electricity, but increafes it in an uncommon degree.

18. Animal electricity is impeded by perfpiration.

19. Animal electricity is ftronger in winter than in fummer.

20. Repofe at noon, or any other fhort fleep when fitting in the day-time, does not difturb pofitive animal electricity.

21. The breath does not conduct animal electricity in a perceptible manner. I breathed on the cover of the condenfer in various places, but I never found the leaft traces of electricity. The electricity, in all probability, was difperfed too fpeedily, or was too fubtile to be perceptible.

22. Bodily motion is by no means neceffary for producing animal electricity;

23. Neither does it depend on the movement occafioned by refpiration.

24. Animal electricity arifes alfo without friction of the clothes. My experiments leave no doubt on this fubject; as I found the electricity on my own body lively and durable for half an hour, or an hour, when I had on no clothes. I do not fay, however, that the friction of the clothes does not increafe it.

25. Animal electricity is excited without any friction of the external parts of the body.

26. There is alfo a fpontaneous kind of animal electricity.

VI. *Observations on Pot-ash; being an Inquiry how far the mischievous Effects of Septic Acid are restrained by Pot-ash and other Alkalies, particularly in respect to the Effects of Septite of Pot-ash (Nitre or Saltpetre) upon Animal Flesh intended to be eaten, and upon the Human Stomach; in a Letter from Dr. Mitchill to Dr. Priestley, dated Plandome, May 4, 1799. Communicated by Dr.* MITCHILL.

ON a former occasion (2 Medical Repository, p. 236 *et seq.*) an attempt was made to shew that the *septic* acid, which is formed in certain putrefactive processes, was materially different in its constitution and qualities from the *nitrous* acid obtained by distillation from saltpetre. Since the composition of that piece, I have observed that Juncker, in his view of the doctrines of Beecher and Stahl, (2 Conspectus Chimie, p. 280,) is of the same opinion, declaring, that in whatever manner the work of separating it from the putrescent or other bodies with which it was naturally mingled, was undertaken, "ne micula tamen acidi nitrosi purè sistitur," *not a particle of pure nitrous acid can be obtained.* And he warns his reader, that in the observations which he offers he means that *spirit of nitre* ONLY which is liberated from its connection with an alkaline salt. On the native septic acid, which is, as he allows, furnished so largely by the animal kingdom, (p. 277,) and is by far the most active and interesting form of oxygenated septon, Juncker, like most other writers, has said scarcely any thing at all.

The highly destructive effects of this offspring of putrefaction have been pointed out in detail in a former essay (1 Medical Repository, p. 39, 40), in which it was observed that certain substances, and among others pot-ash in particular, possessed a power to restrain and curb its ferocity. The neutral salt, formed by the union of this pestilential acid with the fixed alkali, is the saltpetre or nitre of the shops and of commerce. Concerning the medicinal and economical qualities of this substance, it is manifest to me that there are many mistakes yet prevalent: and they appear to be worthy of being pointed out and corrected,

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The ſeptite of pot-aſh has been denominated a ſalt of many excellent qualities, a *ſal polychreſtus*; and a great phyſician once wiſhed, for the good of his profeſſion, there could be found *one* other remedy ſo certain and ſteady in curing diſeaſes as nitre. It has likewiſe been termed an *antiphlogiſtic* remedy, good for all manner of inflammatory diſeaſes with phlogiſtic denſity of the blood; poſſeſſing fine attenuating powers, being in nowiſe acrimonious; and happily calculated to withſtand a putreſcent ſtate of the body. It has further been called a *refrigerant*, a *diuretic*, and a *carminative*, and employed accordingly by thoſe preſcribers who are influenced by the *nominal* efficacy of remedies.

Such are ſome of the ſuperlative effects aſcribed to this compound of the acid of peſtilence and pot-aſh: and for a conſiderable time after I became acquainted with the miſchievous effects, wrought occaſionally by the naked ſeptic acid, I remained in the belief that the ſtrongeſt of the alkalies could hold it faſt, and keep it entirely harmleſs. It therefore did not appear to me improbable that the character of the compound of the two might, as in a multitude of other caſes, be exceedingly different from that of either the conſtituent acid or alkali.

But latterly I have been inclined to the opinion, that pot-aſh is capable of combination with oxygenated ſepton in different degrees; that is to ſay, ſepton, before combining with the alkali, may have been united to *more* or *leſs* of oxygen; and alſo ſepton, in any of its degrees of oxygenation, may be united with pot-aſh in different proportions: in other words, the acid may *vary in its ſtrength*, and likewiſe, on every degree of ſtrength, may be united to the alkali *in various proportion.* The *nitrum nitratum*, deſcribed by the older chemiſts, is an example of pot-aſh *ſuper*-ſaturated with nitric acid, and, ſtrange to tell! has been extolled for its advantageous operation in ardent fevers, accompanied with thirſt and with a dry and foul tongue. I have ſtrong reaſon to think that there is a diſproportion between the acid and alkali in other forms of nitre; as I have known litmus-paper to be turned repeatedly reddiſh by a watery ſolution of ſaltpetre, the reſidue of a quantity which was ſwallowed by

 miſtake,

miſtake, and which nearly deprived a man of his life. A ſet of correct experiments is wanting to elucidate more completely this part of a very curious and highly important ſubject.

Be theſe things as they may, all experience ſhews that the connexion between ſeptic acid and pot-aſh is eaſy to diſſolve; at leaſt, a portion of oxygen ſeparates very readily from the nitre. The experiment of reddening blood by mixing powdered ſaltpetre with it, was known to Hoffman, and, I think, fairly evinces a partial decompoſition of the ſalt. It ſeems to have a ſimilar operation upon the reſidue of that fluid in the fleſh of ſlaughtered animals, and the reddening of the lean and fibrous part of meat is evidently owing to the oxygen attracted from the nitre.

But a heightening of colour is not the only effect which ſeptite of pot-aſh works upon proviſions ſprinkled with it. There are in many meats, eſpecially of old animals, and of thoſe which have been a long time ſalted, a *toughneſs* and *hardneſs* which render them difficult to be cut and to be chewed. The ſeptic acid ſeems in ſome degree to be disjoined from the pot-aſh, and evidently aſſiſts in decompoſing, to a certain point, the vaſcular and fibrous ſtructure of the meat. The quantity of nitre generally put on is ſmall; was the proportion larger, the meat would be yet further diſorganiſed, and be rendered more *ſhort* and *tender*, almoſt even to *rottenneſs*. But the injurious effect of the nitre is prevented by the ſea-ſalt commonly mingled with it, in the manner and upon the principle deſcribed in my "Obſervations on Soda." (2 Medical Repoſitory, p. 292, *et ſeq.*) The uſe of ſaltpetre, then, in curing proviſions, is to make them *reddiſh* and *tender*, and not to exerciſe an *antiſeptic* and *hardening* power, as the muriate of ſoda does.

With all theſe conſiderations before me, I entertained great doubts of the truth of thoſe fine things told over and again, and copied by one writer from another, about the mildneſs and wholeſomeneſs of nitre, and of its wonderful effects as a calmer and ſoother of diſeaſes. It ſeemed very ſtrange to me, that the acid which before its union with pot-aſh was capable of cauſing fevers, ſhould be ſo ſuddenly transformed,

transformed, as, notwithſtanding the laxity of their coheſion, to cure ſuch diſeaſes immediately afterwards.

While I was conſidering theſe things, a caſe fell under my obſervation, which allowed me fully to witneſs the operation of this boaſted *cooler, carminative,* and *febrifuge.* It ſhews, beyond a doubt, that ſeptic acid, though coerced by pot-aſh, is in ſome degree ſeptic acid ſtill. Its native virulence does not even then wholly forſake it. Nitre ought to be ranked among the poiſons; for, in a ſufficient doſe, it is truly a poiſon. Though it may be adminiſtered in ſmall quantities without exciting terrible ſymptoms, it reſembles in that reſpect moſt other venomous ſubſtances, which can be ſwallowed without detriment, and even often with advantage, when their doſe is not too large. It is time for writers of Diſpenſatories, and on Materia Medica, to know the facts concerning nitre and its operation, and to publiſh them for the ſake of undeceiving their readers. Too long has the medical world been the dupe of idle and partial opinions on this ſubject. Read the following accident, and judge of what I have ſaid from the ſymptoms induced by ſwallowing ſeptite of potaſh or ſaltpetre:—

A carman, of middle age, had followed his buſineſs as uſual on Saturday the 20th day of April 1799. On Sunday morning he determined to take a ſaline cathartic for an indiſpoſition too trifling to require the advice of a phyſician. Accordingly his wife diſſolved an ounce of what ſhe believed to be ſulphate of ſoda (Glauber's ſalt) in water, and gave it to him at a draught. He ſwallowed it. Soon after he was ſeverely incommoded by what he termed "a great weakneſs about his heart." Nauſea came on, and was followed by vomiting of the contents of the ſtomach, mixed with conſiderable quantities of blood at each time. His ſtrength was exceedingly impaired: and a ſenſation of coldneſs over the whole body was remarked by him, particularly in the extremities. I did not ſee him until after the bloody vomiting had continued at times for ſeveral hours, and then his pulſes at the wriſt were very ſlow. He was however quite rational, and ſaid he then felt much better than he had done. On examining what kind of ſalt he had taken, for ſome cryſtals of it remained in a wide-mouthed bottle, I found it to be ſep-

tite

tite of pot-aſh (ſaltpetre.) This was about eleven o'clock before noon, and he had taken it at ſix in the morning. As it had had no purgative effect, I ordered him ſome caſtor-oil, and almond milk ſweetened with ſugar, or ſome milk-whey, and ſome water-gruel.

In 35 Commentar. de Rebus, &c. p. 176, a caſe is mentioned of death from taking an ounce of nitre: and if a part of the quantity which this man took had not been vomited up, he probably would have died too. The ol. ricini purged him gently, and he gradually got better, but complained very much of weakneſs about the præcordia.

There are numerous other accidents not materially unlike theſe. Such occurrences give us no very favourable account of the benignity of nitre as a medicine. It is a pity that practiſers of phyſic do not better underſtand the component parts of their preſcriptions. How few know that, in adminiſtering nitre, their patients are made to ſwallow a portion of the nauſeating and ſickening acid of putrefaction!

Poſſibly theſe remarks may have a tendency to remove the doubt contained in your letter of April 11, 1799. You will hereby perceive that *my native acid of ſepton* is a combination of this baſis with oxygen and water; whereas your *artificial acid of nitre* undergoes a partial decompoſition by the heat of diſtillation, and is adulterated beſides with whatever happens to be mingled with it during and after its combination with the vegetable fixed alkali. And both theſe forms of acid differ from atmoſpherical air; inaſmuch as the former are *chemical* mixtures, the latter is *mechanical.*

It would be better for ſcience if the word "nitre" was rejected altogether from uſe. *Nitria,* whence the term comes, was, you know, a diſtrict of ancient Egypt, famous for the quantity of mineral alkali which it afforded. (D'Anville's Geograph.—Egypt.) This ſaline ſubſtance has thence been called by the names *nitrum, nitre,* &c. In confirmation of which, I obſerve, in the Dictionary of Calepinus, printed at Baſil in 1538, that what they called *nitrum* was a material employed to cleanſe clothes, and waſh the bodies which wore them. And S. Bochart remarks, (1 Opera. Chanan, L. II. cap. xiv.) that the ancients made a kind of ley from *aſhes,*

ſoda,

ſoda, and *bole*, (*cinere*, *nitro*, et *cimoliâ*,) for more effectually clearing their bodies from naſtineſs when they bathed. I need not remark to you, that I employ the word "nitre," not in its ancient, but modern ſenſe.

It is highly deſirable that ſome of our men, whoſe opinions have weight with the public, would peruſe the work of Lanciſi, phyſician to Pope Clement XI, on the noxious exhalations of marſhes. (De Nox. Palud. Effluviis.) By the perſevering and luminous reſearches of this great man, it was found, as long ago as the year 1716, that marſh-water, by ſimple diſtillation, (*Ibid.* Lib. II. c. xii.) manifeſted an *acid* quality; and that *calcareous* ſtones (*Ibid.* Lib. II. c. ii.) were better for paving the ſtreets of cities than *ſiliceous* ones, becauſe the *alkaline nature* of the former was adapted to imbibe the noxious moiſture of the air, and ſweeten the *acid ſalts* with which it abounded. Indeed, much of the matter detailed by the writers of our days on local ſources of diſtempers, may be found better obſerved, and better ſtated, by Lanciſi, than in their writings. By the by I obſerve he mentions the Engliſh philoſopher Mayow, (*Ibid.* Lib. II. P. ii. cap. 2.)

If the philoſopher of Rome had reaſoned upon his own diſcovery, he could not have failed to draw the inference, that by *alkaline ſubſtances* might the HYDRA of peſtilence be overcome.—As I have mentioned this monſter, I ſhall pauſe a little to give you my opinion of the allegory among the ancients concerning her: and I attempt the explanation the more willingly, as I believe Lord Verulam has ſaid nothing about it. The fable is this:—In Peloponneſus, between Mycenæ and Argos, there was a fen or marſh of ſome extent called Lerna. This muddy and ſtagnating pool was inhabited by Hydra, a horrible and devouring monſter with ſeveral heads; ſome ſay ſeven, others nine, and others fifty. The malignity of her poiſon was ſuch that a wound from an arrow dipped into it was inſtantly mortal. She made dreadful havoc among the people of the ſurrounding country, and devoured a great number of their ſheep and other cattle. In obedience to the orders of the tyrant Euryſtheus, Hercules went to fight this deſtructive and formidable creature. On

his

his approach, a crab came forth to the affiftance of Hydra: but Hercules crufhed the crab, and afterwards flew Hydra. Of the heads of Hydra, it was reported, when one was cut off, two would fprout from the wound, unlefs prevented by the immediate application of fire. Hercules, availing himfelf of the aid of fire, fucceeded in his undertaking. In the ninth figure of Montfaucon's 66th plate, there is a figure of Hercules with crabs near his feet, having, as the learned father curioufly enough remarks, a relation to fome myftery which he does not comprehend. (1 Antiquity explained, Art. Hercules, chap. ix.)

Now, it appears to me, this is an allegory expreffive of the peftilential vapours emitted by the bog of Lerna, and of the means found by experience ufeful to drain off its ftagnant water, and to clear the adjoining and furrounding moraffes.

The word "hydra" is derived from ὕδωρ, *water*. This fluid then, detained upon the marfh of Lerna, favoured occafionally the production of unwholefome exhalations. Such vapours, being at once invifible and injurious, were afcribed to fome preternatural enemy or deftructive monfter; and being diffufed, or wafted around the country, and oftentimes cutting off both man and beaft, were fancied to be the effect of the fuppofed monfter's poifon. According to their extent and virulence was fhe reported to have fewer or more heads for preparing and inflicting this poifon. The mere draining off the water, and leaving the mud and flime bare, was termed cutting off an head; and the increafe of deleterious gafes, in confequence of expofing fuch a naked furface, was aptly expreffed by the fprouting forth of two in its place. By cauterifing, or fearing, was underftood either the folar heat in drying the ground after the water was drained away, or the burning up of the trees, fhrubs, and obftacles to free ventilation by ordinary combuftion, or perhaps both. The crab, who was Hydra's ally, perhaps does not refer to the fun's place in the conftellation Cancer, fo much as to fhew the frequent recurrence of the difficulties, and the fuperior ftrength and fkill requifite to overcome them. In the whole allegory "Hercules" may be underftood to mean "infuperable courage and induftry." North America, at the clofe of the 18th

 century,

century, wants a Hercules. This interpretation is confirmed by another conſideration, that the ancients had not only their *Hydra*, who lived in the water, but their *Cherſydra*, who remained after the marſh or fen was dried up. *Cherſydra*, being derived from the two words, χερσος, *land not fit for the plough*; and υδρα, *the monſter of the fens*; will thus mean the venomous and ſickly condition of the neighbouring atmoſphere after the water was exhaled, and the ground at the ſame time not rendered arable thereby, typified by a poiſonous ſerpent: and was thus expreſſive of the rage of peſtilential effluvia, which ſometimes, and under certain circumſtances, continue in a virulent ſtate, in dry weather, near their dried ſources.

Hydra is ſeemingly mentioned by Virgil (Æn. vi. v. 576.) as a *fictitious* or *poetical* animal. Bochart, however, with his uſual prodigious erudition, appears deſirous to make the whole ſtory *literally* intelligible. (Hierozoici pars poſter. Lib. III. cap. xiii.)* But Lanciſi, with a more clear and diſcriminating mind, perceives that important phyſical truths are concealed under this two-fold allegory, and ſhews how they are to be unriddled. (De Nox. Palud. Effl. Lib. II. p. ii. cap. 3.)—In conſidering theſe matters you will not fail to recollect that the claſſical writers, and others, uſe the word "hydrus" as well as "hydra," and ſome of them apparently confound the two. The former noun of the maſculine gender is probably the name of the *real* animal, the *water-ſnake*, the latter of the *imaginary* one.—But of this enough.

If peſtilential matter, as I obſerved before, can be ſubdued by alkalies, then the formation of ſeptite of pot-aſh in the alimentary canal muſt be a very frequent and common proceſs. It is univerſally agreed that peſtilential matter may be taken into the ſtomach by ſwallowing; and no reaſonable doubt can be entertained of its production within that organ, and other parts of the inteſtinal tube, from the corrupted remains of food. The whole tenor of preſcription, as explained in my letter concerning the uſe of alkaline remedies in fevers, (1 Medical Repoſ. p. 265.) goes to ſhew that the offending cauſe is of an acid quality: and in its worſt forms, I think he acid engendered has ſepton for its baſis; ſince aliment,

 containing

containing this principle only, is capable of yielding that most noxious compound. When septic acid thus exists in the alimentary canal, and carbonates, tartrites, &c. of pot-ash, are taken down, the stronger acid is attracted, and a proportional quantity of nitre or saltpetre is formed. Thus, in abundance of cases, the practisers who are fond of prescribing septite of pot-ash might spare themselves the trouble, as that saline compound is, in certain cases, formed in the stomach and bowels in considerable quantity. What precise effect the *nitre so formed* has on the intestines, and on the constitution at large, deserves to be inquired into with great care; for I am not without apprehension that some of the sad symptoms, occasionally attendant on fevers, are modified by the septite of pot-ash formed within the belly.—I believe, however, I must now stop, and trouble you no further for the present than by assuring you, that I hope much from your aid in this inquiry, which you inform me you consider "as a happy train of discovery;" and that I am, very respectfully, yours,

SAM. L. MITCHILL.

To Dr. Priestley.

VII. *Description of a Mercurial Gazometer constructed by Mr.* W. H. Pepys *jun. Communicated by the Author.*

THE difficulty attending the exhibition of acid and alkaline gases, was the first inducement to Priestley, Lavoisier, and others, to use mercury for such experiments; but the great expence and enormous weight of this fluid obliged many accurate and experienced chemists to relinquish them almost entirely, as there appeared no other likely means of succeeding but by its means and that of the exhausted receiver. A contrivance to lessen the quantity of mercury necessary for such experiments was therefore a desirable object; and by introducing the dome used by Mr. Watt in his hydraulic bellows, I have succeeded in constructing an apparatus in which both of the above-mentioned requisites are obtained at a comparatively small expence. As it is hardly possible

poſſible to japan tin or copper ſo perfectly as to prevent their being attacked by mercury when brought in contact with them, I had the cylinders turned in lignum vitæ, on which the mercury has no action : the conducting tube is of glaſs ; and the cocks are coated on the inſide with varniſh.

The uſefulneſs of an apparatus ſo conſtructed will appear ſufficiently obvious, when it is recollected that all gaſes paſſed through any other fluid than mercury, water for inſtance, take up a quantity of moiſture, which adds conſiderably to their gravity, and makes it impoſſible to determine their real weight.

For weighing of gaſes I make uſe of a glaſs globe and ſtop-cock of a ſmaller ſize than is commonly employed, as greater accuracy can be obtained by uſing a proportionally delicate beam than by employing a larger globe, which muſt be ſuſpended to a beam of ſuch ſtrength as greatly to increaſe the friction on its axes. It is of great importance in the analyſis of bodies, or in other chemical experiments, to be able to aſcertain with accuracy the weight of any gas obtained by the proceſs. The weight of two or more quantities of gas ſhould however be always tried, and the mean be taken to prevent any error.

A, (Plate III.) is a repreſentation of the bell of the gazometer, made of glaſs, furniſhed with a cock at top, and able to contain 34 ounces troy of diſtilled water. The diviſions of capacity, determined by actual meaſurement, are marked on the glaſs with a diamond. BB, ſection of two cylinders of lignum vitæ, the outward one ſcrewed upon the ſolid internal one, which is made to project at its lower extremity, and furniſhed with a male ſcrew, to work into a female ſcrew with which the lower end of the external cylinder is furniſhed. The ſpace between theſe is ſo adjuſted as to be almoſt filled up by the ſubſtance of the glaſs bell A when dropped into it, ſo that the quantity of mercury neceſſary to fill up that ſpace is proportionally ſmall. The internal cylinder has a conducting tube up through its axis, the lower end of which is furniſhed with a female ſcrew anſwering to the male ſcrew of the cock of the ſmall receiver C. The receiver C is made of glaſs, and open at bottom. When this

receiver is ufed, it is fcrewed into its place, and refts upon a fmall cup or ciftern of mercury D, in which the beak of a retort, furnifhed with a bent glafs tube, to be afterwards noticed, may be introduced under the receiver. E,E,E,E, fection of a wooden ftand upon which the cylinders of lignum vitæ are fupported, having an opening through the top to permit the cock of the receiver C to be joined to the conducting tube of the internal cylinder B. The ciftern D is adjufted to its height by means of a rifing cylinder in the pedeftal F. G is a transfer glafs for mixing alkaline gafes in vacuo, or other purpofes; and, when ufed, is joined to the top of the bell A. H, a glafs globe and ftop-cock, capable of holding 14 ounces troy of diftilled water for weighing gafes: it receives its gas by being inverted, and fcrewed into the bell A. I, a bladder furnifhed with a ftop-cock to affift in holding, transferring, or mixing different gafes. K, an elaftic gum-bottle, capable of containing 30 ounces of diftilled water, for holding the acid gafes: when ufed, it is fcrewed into the top of the transfer G; the bottom cock of the latter being at the fame time joined to the bell A, previoufly charged with the alkaline gas: the cocks being turned, the gafes rufh together in vacuo. L, a fmall portable air-pump, for exhaufting the globe H. M, a double male fcrew, which fits any part of the apparatus, and on which a valve may be faftened. N, a double female fcrew. O, a fmall inftrument, which I would not have mentioned, had I not found it of peculiar fervice in collecting fpilled mercury: it is made of glafs, the mouth being applied at *a*: you may collect any fmall globules of mercury by the fmall end *b*, by which means they are elevated into the receiver at Q. It is ufeful likewife for removing mercury from the fmall ciftern.

One of the principal objections to the ufe of mercury in fuch experiments as this apparatus is intended for, has been, the great force neceffary to overcome the refiftance of a column of mercury when gafes are to be received over that denfe fluid; a refiftance in the proportion of one inch of mercury to fourteen inches of water, and which very few lutes are able to withftand. This refiftance I overcome by a very fimple contrivance: a bent tube fitted into the beak

of

of the retort, (if one be employed,) or into a Wolf's apparatus, and paſſing into the upper part of the ſmall receiver, as expreſſed in the plate at C. By employing mercury for ſuch experiments, another advantage is gained by the uſe of this apparatus, namely, a power of exhauſtion in the retort, or Wolf's bottle, equal to a column of two inches of mercury, or 28 inches of water. This will be eaſily conceived when it is recollected that, by drawing up the large receiver A, the ſmall one C is raiſed in its ciſtern, bearing up with it the contained mercury, which is kept in its place by the preſſure of the atmoſphere on the ſurface of the mercury in the ciſtern. The cock of the ſmall receiver C is then to be turned off, and that of the large one A to be turned on. The air, of which the retort, or Wolf's bottle, is thus exhauſted, may then be let out, by plunging A into the mercury between the cylinders BB, and turning off the cock. When a ſufficient quantity of gas paſſes from the retort, or bottle, through the bent tube into C, to level the mercury in it and the ciſtern, the communication may again be opened, and the ſame ſteps followed as before deſcribed. By this means I have been enabled to obtain more gas, from the ſame materials, than if I had received it through a fluid of the weight of water; a circumſtance of ſome importance where nice and accurate reſults are looked for.

The plate of the apparatus is on a ſcale of nearly three inches to a foot.

VIII. *A Communication reſpecting the Preparation of Writing Ink. By Mr.* DESORMEAUX *junior.*

Mr. EDITOR,

BEING a conſtant reader of your truly inſtructive and entertaining Work, and having noticed in the Number for October, p. 29, a communication relative to an improved writing ink, I am, in conſequence, induced to offer ſome practical remarks on that ſubject.

Previous to my reading that communication reſpecting the diſcoveries of Prouſt having been applied by Van Mons to

to the preparation of the fluid alluded to, I had made a great variety of experiments, in some of which I treated the sulphat of iron precisely as there directed; but since that time, I have, with a view to improvement, followed the rule which is there prescribed strictly, and have besides, at former intervals, gone over and varied the experiments of Macquer, Lewis, Berthollet, Ribancourt, Proust, and other celebrated chemists, both foreign and native, who have treated on this and other subjects nearly allied to it; with a view, if possible, to establish certain data upon which to ground the best mode of fabricating the article in question, being very desirous constantly to obtain an ink for the common purposes of penmanship, which, at the same time that it should flow with freedom, should have the proper degree of lustre or glossiness, and be at the moment of using intensely black, with a capability of retaining that colour continually, even though openly exposed to the action of the sun and air. This, I presume, I have accomplished: at least, during a space of ten years, I have never met with any formula which has so well rewarded my pains as the one which I am about to communicate. Having been brought up in a line of business in which I am in the daily habit of observing the action of such substances upon each other as enter into the formation of ink, it may be readily imagined that, in a practical point of view, my opportunities for the improvement of it have fallen little short of any individual whatever. If to this I add, that, since the year 1794, I have annually supplied the public with at least 17,000 gallons of ink, besides preparing powder for its occasional production, it may be inferred that I have spared no pains in studying what would best conduce to its perfectability; and after the many attempts which I have made, I have never found my expectation or that of my friends deceived, when the process has been conducted agreeably to the following directions:—In six quarts (beer measure) of water, (it does not appear of importance whether it be rain, river, or spring water,) *boil* four ounces of the best Campeachy logwood, chipped very thin across the grain; (the boiling may be continued near an hour;) adding from time to time a little *boiling* water, to compensate for waste by evaporation: strain

9 the

the liquor *whilst hot*, suffer it to cool, and make up the quantity equal to five quarts by the further addition of cold water. To this *cold* decoction put one pound averdupois weight of blue galls, or 20 ozs. of the best galls in sorts, which should be first coarsely bruised; 4 ozs. of sulphat of iron, calcined to whiteness; $\frac{1}{2}$ oz. of the acetite of copper, which should be triturated in a mortar, moistened by a little of the decoction gradually added till it be brought to the form of a smooth paste, and then thoroughly intermixed with the whole mass. Three ounces of coarse brown sugar, and six ounces of good gum Senegal, or Arabic, are also to be added. These several ingredients may be introduced one after the other immediately, contrary to the advice of some, who recommend the gum, &c. to be added when the ink is nearly made; as gum, however, is at present exorbitantly dear, three or four ounces will be found sufficient, with only one and an half ounce of sugar, unless, for particular purposes, it is wanted to bear a higher gloss than common. In regard to the use of sugar, which I have here recommended, I hope I shall not trespass in remarking, that my observations, on a very extensive scale, are coincident with those of M. Ribancourt, who says, that a degree of fluidity is by its means imparted, which allows the dose of gum to be enlarged considerably beyond what it would bear without it; and it is thereby rendered less liable to clog the pen, which, especially when the nib is very fine, if it does not flow freely, quits it so slow as unpleasantly to retard the writer, and is totally unfit for fine manuscripts; besides, by such an union of gum and sugar, a degree of consistency is given to the liquor, which enables it to suspend a much greater portion of colouring matter than otherwise could be effected; a circumstance of the greatest importance to its hue and permanency. It is far from my design, in thus transmitting my ideas, to induce a supposition that it is *exactly* after this manner that I, or any other person who supply ink by wholesale, compound it; so far from it, that the first cost of one gallon, on the plan here suggested, would exceed twice the sum which is usually paid for it to the manufacturer, although by retail it is disposed of at an enormous rate. For private persons, therefore, who wish to be

be particular in their manuſcripts, the rule I have here laid down will be found fully to anſwer their expectations at a cheaper rate than they are in general furniſhed with it. The beſt veſſel, in my opinion, in which to make ink, is a common ſtone bottle, which will contain half as much more as is propoſed to be made, and which ſhould be agitated twice a day, and be left uncorked, in order to expoſe freſh ſurfaces of the liquor to the action of the air, without which it cannot be expected to write very black when firſt uſed, but with this precaution may be fit for uſe in about 14 days; when it may be poured from its dregs, and corked up, to preſerve it from duſt, as well as waſte by evaporation. In cold weather it ſhould never be ſuffered to freeze, which is found to cauſe a diſunion of its parts, nor does it ever afterwards recover its former intenſity of colour, luſtre, or durability. Many perſons, I am aware, diſapprove, and therefore omit the uſe of logwood, from an apprehenſion that it induces a tendency in the ink to fade; this however, from the ſtricteſt attention, I can aſſert to be fallacious, at leaſt where it is not had recourſe to by way of ſubſtitute for the other more expenſive articles; indeed its effects are quite the reverſe, where all the ingredients are properly proportioned. As to the beautiful complexion which is given by it to the entire maſs of fluid, it would be needleſs to inſiſt, unleſs for the ſake of thoſe who are unacquainted how a mixture of the acetite of copper and logwood liquor work together, and by which, in this inſtance, a richneſs and bloom is given, which can in no other way be got with equal economy and ſucceſs; independent of which, the colouring matter of the wood, by its affinity with the oxyde of iron, has a very powerful effect to blacken the ink, and to render it leſs capable of change from any unſaturated acid in the ſulphat of iron, or from the operation of the air. I am aware, too, that the introduction of cupreous matter has been objected to, on the ſcore of its injuring the penknife by a portion of copper attaching itſelf thereto in conſequence of ſuperior elective attraction, whereby it has been remarked that a part of the knife's edge is diſſolved, and a quantity of copper anſwering thereto is exchanged and depoſited in its place, which is always ſaid to happen when the pen retains

a little

a little ink at the time of its being mended. This, I think, is ſpinning out the effect of chemical affinity too far; for although, with reſpect to the reaſoning, the truth muſt be admitted, I contend, notwithſtanding, that its conſequences to the edge are not apparent to the eye; nor is any ſenſible detrimental change thereby produced upon the knife, more than what is induced by any other ink into which neither the ſulphat or the acetite of copper is introduced. As to myſelf, however, I have never yet ſeen a receipt for the formation of ink, where each of the principles were ſo completely proportioned and ſaturated with each other as to be inert when applied to the ſurface of a piece of poliſhed metal, ſuch as iron, for example; nay, even the frequent contact of water only, if allowed to remain, would ſpoil any inſtrument made of that material. I ſhall not enlarge on this particular, conceiving the moſt powerful objection I can offer in oppoſition to that opinion, is, that I have now lying by me a penknife, which has been in conſtant uſe between two and three years, and which even yet retains almoſt as good an edge as it had at firſt, by the mere aſſiſtance of an unprepared leather ſtrap, although in the ink with which I write the acetite of copper has always been uſed. But even admitting, for argument's ſake, that what I have attempted to diſprove is true, how many hundred pens might be made and mended ere it would be requiſite to beſtow twopence to the cutler for the exerciſe of his art? In ſhort, if the introduction of the ſubſtance alluded to into ink be a fault, I conſider it as one of no conſequence when put in competition with the advantages of beauty, durability, and intenſity of colour imparted by it.

I ſhall intrude no longer on the time or patience of your readers, than by requeſting, if any of them are poſſeſſed of a cheap and efficacious method whereby the mouldineſs of ink may be prevented, that they will communicate the ſame through the medium of your Magazine, in doing which a very conſiderable ſervice will be rendered towards its perfectability. The admixture either of a ſmall quantity of the muriate of ſoda, of the nitrate of pot-aſh, of alcohol, or of cloves, have been ſeverally recommended for this purpoſe; but by experience I know that of theſe only alcohol will

avail, and this cannot be added in fufficient quantity without caufing it to fink into, and fpread upon the paper; fo that, indeed, *its* ufe is altogether interdicted. The beft plan which I have hitherto found as a preventive to the vegetative procefs I here allude to, is, to add the ingredients of which the ink is compofed to the *cold* decoction; if recourfe be had to boiling, ALL *together*, it is found very rapidly to promote the inconvenience; and were ebullition totally avoided, it would on that account be better, but then we could not obtain the ink of fo deep a colour; yet I know of no inftance in which mouldinefs will not appear in time, and that in no inconfiderable degree. In fine, I am ignorant of any fubftance, or method, that has been hitherto ufed as a fpecific to remedy the defect. If there is any that can be fuggefted, I fhould be happy to put it to the teft of experiment.

I am, Sir, yours, &c.

L. DESORMEAUX jun.

No. 8, Vine Court, Spitalfields.

IX. *An Inquiry concerning the Weight afcribed to Heat. By* BENJAMIN COUNT RUMFORD, *F.R.S. M.R.I.A.* &c. *Read before the Royal Society May 2,* 1799.

THE various experiments which have hitherto been made with a view to determine the queftion fo long agitated, relative to the weight which has been fuppofed to be gained, or to be loft, by bodies upon their being heated, are of a nature fo very delicate, and are liable to fo many errors, not only on account of the imperfections of the inftruments made ufe of, but alfo of thofe, much more difficult to appreciate, arifing from the vertical currents in the atmofphere, caufed by the hot or the cold body which is placed in the balance, that it is not at all furprifing that opinions have been fo much divided relative to a fact fo very difficult to afcertain.

It is a confiderable time fince I firft began to meditate on this fubject, and I have made many experiments with a view to its inveftigation; and in thefe experiments I have taken all

all those precautions to avoid errors, which a knowledge of the various sources of them, and an earnest desire to determine a fact which I conceived to be of importance to be known, could inspire: but though all my researches tended to convince me, more and more, that *a body acquires no additional weight upon being heated*, or rather, that heat has no effect whatever upon the weights of bodies, I have been so sensible of the delicacy of the inquiry that I was for a long time afraid to form a decided opinion upon the subject.

Being much struck with the experiments recorded in the Transactions of the Royal Society, Vol. LXXV. made by Dr. Fordyce, upon the weight said to be acquired by water upon being frozen; and being possessed of an excellent balance, belonging to his most Serene Highness the Elector Palatine Duke of Bavaria, early in the beginning of the winter of the year 1787 (as soon as the cold was sufficiently intense for my purpose) I set about to repeat those experiments, in order to convince myself whether the very extraordinary fact related might be depended on; and with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner:—

Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask (blown as thin as possible) and of the same shape and dimensions, I chose out from amongst them two, which, after using every method I could imagine of comparing them together, appeared to be so much alike as hardly to be distinguished.

Into one of these bottles, which I shall call A, I put 4107,86 grains troy of pure distilled water, which filled it about half full; and into the other, B, I put an equal weight of weak spirit of wine; and, sealing both the bottles hermetically, and washing them and wiping them perfectly clean and dry on the outside, I suspended them to the arms of the balance, and placed the balance in a large room, which for some weeks had been regularly heated every day by a German stove, and in which the air was kept up to the temperature of 61° of Fahrenheit's thermometer, with very little variation. Having suffered the bottles, with their contents, to remain in this situation till I conceived they

muſt have acquired the temperature of the circumambient air, I wiped them afreſh with a very clean dry cambric handkerchief, and brought them into the moſt exact equilibrium poſſible, by attaching a ſmall piece of very fine ſilver wire to the arm of the balance, to which the bottle which was the lighteſt was ſuſpended.

Having ſuffered the apparatus to remain in this ſituation about twelve hours longer, and finding no alteration in the relative weights of the bottles, (they continuing all this time to be in the moſt perfect equilibrium,) I now removed them into a large uninhabited room fronting the north, in which the air, which was very quiet, was at the temperature of 29° F., the air without doors being at the ſame time at 27°; and, going out of the room, and locking the door after me, I ſuffered the bottles to remain forty-eight hours, undiſturbed, in this cold ſituation, attached to the arms of the balance as before.

At the expiration of that time I entered the room, uſing the utmoſt caution not to diſturb the balance, when, to my great ſurpriſe, I found that the bottle A very ſenſibly preponderated.

The water which this bottle contained was completely frozen into one ſolid body of ice; but the ſpirit of wine, in the bottle B, ſhewed no ſigns of freezing.

I now very cautiouſly reſtored the equilibrium, by adding ſmall pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was ſuſpended, when I found that the bottle A had augmented its weight by $\frac{1}{35904}$ part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811,23 grains troy, (the bottle weighing 703,37 grains, and the water 4107,86 grains,) and it requiring now $\frac{134}{1000}$ parts of a grain, added to the oppoſite arm of the balance, to counterbalance it.

Having had occaſion juſt at this time to write to my friend, Sir Charles Blagden, upon another ſubject, I added a poſtſcript to my letter, giving him a ſhort account of this experiment, and telling him how "*very contrary to my expectation*" the reſult of it had turned out: but I ſoon after found that I

had

had been too hasty in my communication. Sir Charles, in his answer to my letter, expressed doubts respecting the fact; but, before his letter had reached me, I had learned, from my own experience, how very dangerous it is, in philosophical investigations, to draw conclusions from single experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which still remained at the temperature of 61°,) the ice in the bottle A gradually thawed; and being at length totally reduced to water, and this water having acquired the temperature of the surrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment before the water was frozen.

This experiment, being repeated, gave nearly the same result, the water appearing, when frozen, to be heavier than in its fluid state; but some irregularity in the manner in which the water lost the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as also a sensible difference in the quantities of weight apparently acquired in the different experiments, led me to suspect that the experiment could not be depended on for deciding the fact in question: I therefore set about to repeat it, with some variations and improvements:—but, before I give an account of my further investigations relative to this subject, it may not be amiss to mention the method I pursued for discovering whether the appearances mentioned in the foregoing experiments might not arise from the imperfections of my balance; and it may likewise be proper to give an account, in this place, of an intermediate experiment which I made, with a view to discover, by a shorter route, and in a manner less exceptionable than that above-mentioned, whether bodies actually lose, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My suspicions respecting the accuracy of the balance arose from a knowledge (which I acquired from the maker of it) of the manner in which it was constructed.

The three principal points of the balance having been determined, as nearly as possible, by measurement, the axes of

motion were firmly fixed in their places in a right line, and the beam being afterwards finiſhed, and its two arms brought to be in equilibrio, the balance was proved by ſuſpending weights, which before were known to be exactly equal, to the ends of its arms.

If with theſe weights the balance remained in equilibrio, it was conſidered as a proof that the beam was juſt; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, ſuſpended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though in this method of conſtructing balances the moſt perfect equality in the lengths of the arms may be obtained, and conſequently the greateſt poſſible accuracy, when uſed at a time when the temperature of the air is the ſame as when the balance was made; yet, as it may happen that, in order to bring the arms of the balance to be of the ſame length, one of them may be much more hammered than the other, I ſuſpected it might be poſſible that the texture of the metal forming the two arms might be rendered ſo far different by this operation as to occaſion a difference in their expanſions with heat; and that this difference might occaſion a ſenſible error in the balance, when, being charged with a great weight, it ſhould be expoſed to a conſiderable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, aroſe in any degree from this cauſe, I had only to repeat the experiment, cauſing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a ſenſible difference in the reſults of different repetitions of the ſame experiment, made as nearly as poſſible under the ſame circumſtances, and as it was above all things of importance to aſcertain the accuracy of my balance, I preferred making a particular experiment for that purpoſe.

My firſt idea was, to ſuſpend to the arms of the balance, by very

very fine wires, two equal globes of glass filled with mercury, and, suffering them to remain in my room till they should have acquired the known temperature of the air in it, to have removed them afterward into the cold, and to have seen if they still remained in equilibrio under such difference of temperature: but, considering the obstinacy with which moisture adheres to the surface of glass, and being afraid that somehow or other, notwithstanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increased; and having found, by a former experiment, of which I have already had the honour of communicating an account to the Royal Society, that the gilt surfaces of metals do not attract moisture; instead of the glass globes filled with mercury, I made use of two equal solid globes of brass, well gilt and burnished, which I suspended to the arms of the balance by fine gold wires.

These globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature (61°) of my room, in which they were exposed more than twenty-four hours, were brought into the most scrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26°, where they were left all night.

The result of this trial furnished the most satisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts respecting the accuracy of my balance, I now resumed my investigations relative to the augmentation of weight which fluids have been said to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as possible against every source of error and deception. The bottles being of the same size, neither any occasional alteration in the pressure of the atmosphere during the experiment, nor the necessary and unavoidable difference in the densities of the air in the hot and in the cold rooms in which they were weighed, could affect their

their apparent weights; and their ſhapes and their quantities of ſurface being the ſame, and as they remained for ſuch a conſiderable length of time in the heat and cold to which they were expoſed, I flattered myſelf that the quantities of moiſture remaining attached to their ſurfaces could not be ſo different as ſenſibly to effect the reſults of the experiments. But, in regard to this laſt circumſtance, I afterwards found reaſon to conclude that my opinion was erroneous.

Admitting the fact ſtated by Dr. Fordyce, (and which my experiments had hitherto rather tended to corroborate than to contradict,) I could not conceive any other cauſe for the augmentation of the apparent weight of water, upon its being frozen, than the loſs of ſo great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loſs of latent heat added to the weight of one body, it muſt of neceſſity produce the ſame effect on another, and conſequently, that the augmentation of the quantity of latent heat muſt, in all bodies, and in all caſes, diminiſh their apparent weights.

To determine whether this is actually the caſe or not, I made the following experiment:—

Having provided two bottles, as nearly alike as poſſible, and in all reſpects ſimilar to thoſe made uſe of in the experiments above-mentioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and ſealing them hermetically, and ſuſpending them to the arms of the balance, I ſuffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at the temperature of 34°, where they remained twenty-four hours. But there was not the leaſt appearance of either of them acquiring or loſing any weight.

Here it is very certain that the quantity of heat loſt by the water muſt have been very conſiderably greater than that loſt by the mercury, the ſpecific quantities of latent heat in water and in mercury having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat loſt, produced no ſenſible difference on the weights of the fluids in queſtion.

Had

Had any difference of weight really existed, had it been no more than *one millionth* part of the weight of either of the fluids, I should certainly have discovered it; and, had it amounted to so much as $\frac{1}{750000}$ part of that weight, I should have been able to have measured it: so sensible, and so very accurate, is the balance which I used in these experiments.

I was now much confirmed in my suspicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, arose from some accidental cause; but I was not able to conceive what that cause could possibly be,—unless it were either a greater quantity of moisture attached to the external surface of the bottle which contained the water, than to the surface of that containing the spirits of wine,—or some vertical current or currents of air, caused by the bottles, or one of them, not being exactly of the temperature of the surrounding atmosphere.

Though I had foreseen, and, as I thought, guarded sufficiently against these accidents, by making use of bottles of the same size and form, and which were blown of the same kind of glass, and at the same time, and by suffering the bottles in the experiments to remain for so considerable a length of time exposed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice, and of spirit of wine, with respect to heat; or, in other words, the degrees of facility or difficulty with which they acquire the temperature of the medium in which they are exposed, or the time taken up in that operation; and, consequently, was not *absolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with such variations as should put the matter in question out of all doubt.

I was the more anxious to assure myself of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by causing unequal currents in the air, but also by causing, at the same time, a greater or less quantity of moisture to remain attached to the glass.

To remedy theſe evils, and alſo to render the experiment more ſtriking and ſatisfactory in other reſpects, I proceeded in the following manner:—

Having provided three bottles, A, B, and C, as nearly alike as poſſible, and reſembling in all reſpects thoſe already deſcribed; into the firſt, A, I put 4214,28 grains of water, and a ſmall thermometer, made on purpoſe for the experiment, and ſuſpended in the bottle in ſuch a manner that its bulb remained in the middle of the maſs of water; into the ſecond bottle, B, I put a like weight of ſpirit of wine, with a like thermometer; and, into the bottle C, I put an equal weight of mercury.

Theſe bottles, being all hermetically ſealed, were placed in a large room, in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and, being ſuffered to remain in this ſituation more than twenty-four hours, the heat of the room (61°) being kept up all that time with as little variation as poſſible, and the contents of the bottles A and B appearing, by their incloſed thermometers, to be exactly at the ſame temperature, the bottles were all wiped with a very clean, dry, cambric handkerchief; and, being afterwards ſuffered to remain expoſed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat, or of the moiſture attached to their ſurfaces, which might have been occaſioned by the wiping, might be corrected by the operation of the atmoſphere by which they were ſurrounded, they were all weighed, and were brought into the moſt exact equilibrium with each other by means of ſmall pieces of very fine ſilver wire attached to the necks of thoſe of the bottles which were the lighteſt.

This being done, the bottles were all removed into a room in which the air was at 30°, where they were ſuffered to remain, perfectly at reſt and undiſturbed, forty-eight hours; the bottles A and B being ſuſpended to the arms of the balance, and the bottle C ſuſpended, at an equal height, to the arm of a ſtand conſtructed for that purpoſe, and placed as near the balance as poſſible, and a very ſenſible thermometer ſuſpended by the ſide of it.

At the end of forty-eight hours (during which time the apparatus was left in this situation) I entered the room, opening the door very gently for fear of disturbing the balance; when I had the pleasure to find the three thermometers, *viz.* that in the bottle A, which was now inclosed in a solid cake of ice, that in the bottle B, and that suspended in the open air of the room, all standing at the same point, 29° F., and the bottles A and B *remaining in the most perfect equilibrium.*

To assure myself that the play of the balance was free, I now approached it very gently, and caused it to vibrate; and I had the satisfaction to find, not only that it moved with the utmost freedom, but also, when its vibration ceased, that it rested precisely at the point from which it had set out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewise remained of the same apparent weight as at the beginning of the experiment, being in the same perfect equilibrium with the bottle A as at first.

I afterwards removed the whole apparatus into a warm room, and, causing the ice in the bottle A to thaw, and suffering the three bottles to remain till they and their contents had acquired the exact temperature of the surrounding air, I wiped them very clean, and, comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated several times, and always with precisely the same result; the water, *in no instance*, appearing to gain or to lose the least weight upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the least changed, by the various degrees of heat, and of cold, to which they were exposed.

If the bottles were weighed at a time when their contents were not *precisely of the same temperature*, they would frequently appear to have gained, or to have lost, something of their weights; but this doubtless arose from the vertical currents which they caused in the atmosphere upon being heated or cooled in it; or to unequal quantities of moisture attached to the surfaces of the bottles; or to both these causes operating together.

As I knew that the conducting power of mercury, with respect to heat, was considerably greater than either that of water or that of spirit of wine, while its capacity for receiving heat is much less than that of either of them, I did not think it necessary to inclose a thermometer in the bottle C, which contained the mercury; for it was evident that when the contents of the other two bottles should appear, by their thermometers, to have arrived at the temperature of the medium in which they were exposed, the contents of the bottle C could not fail to have acquired it also, and even to have arrived at it before them; for, the time taken up in the heating or in the cooling of any body, is, *cæteris paribus*, as the capacity of the body to receive and retain heat *directly*, and as its conducting power *inversely*.

The bottles were suspended to the balance by silver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not to touch the glass. I likewise avoided, upon all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight upon being changed from a state of *fluidity* to that of *ice*, and *vice versa*, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, from the results of the above-mentioned experiments, that if heat be in fact a *substance*, or matter, (a fluid *sui generis*, as has been supposed,) which, passing from one body to another, and being accumulated, is the immediate cause of the phænomena we observe in heated bodies, (of which, however, I cannot help entertaining doubts,) it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in nowise be affected by such motion.

It

It is, no doubt, upon the supposition that heat is a substance distinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken with a view to determine the weight which bodies have been supposed to gain, or to lose, upon being heated or cooled, have been made; and upon this supposition (but without, however, adopting it entirely, as I do not conceive it to be sufficiently proved,) all my researches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable; in which no foreign cause whatever could affect the results of them; and the quantity of heat which water is known to part with, upon being frozen, is so considerable, that if this loss has no effect upon its apparent weight, it may be presumed that we shall never be able to contrive an experiment by which we can render the weight of heat sensible.

Water, upon being frozen, has been found to lose a quantity of heat amounting to 140 degrees of Fahrenheit's thermometer; or, which is the same thing, the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses, upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the given temperature, (that of freezing,) would heat it 140 degrees, or would raise it to the temperature of ($32^\circ + 140$) 162° of Fahrenheit's thermometer, which is only 60° short of that of boiling water; consequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loses almost as much heat as, added to it, would be sufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the water in its fluid state, and heated to the temperature of 61° F., and by the ice, in the experiments before-mentioned, was *at least* nearly equal to that between water in a state of boiling, and the same at the temperature of freezing.

But this quantity of heat will appear much more considerable, when we consider the great capacity of water to contain heat, and the great apparent effect which the heat that water loses upon being frozen would produce, were it to be imbibed

bibed by, or communicated to, any body whofe power of receiving and retaining heat is much lefs.

The capacity of water to receive and retain heat, or what has been called its fpecific quantity of latent heat, has been found to be to that of gold as 1000 to 50, or as 20 to 1; confequently, the heat which any given quantity of water lofes upon being frozen, were it to be communicated to an equal weight of gold at the temperature of freezing, the gold, inftead of being heated 162 degrees, would be heated $140 \times 20 = 2800$ degrees, or would be raifed to a *bright red heat*.

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about $9\frac{3}{4}$ oz.) of gold would require to heat it from the temperature of freezing water to be *red hot*, has no fenfible effect upon a balance capable of indicating fo fmall a variation of weight as that of $\frac{1}{1000000}$ part of the body in queftion; and if the weight of gold is neither augmented nor leffened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very fafely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES WILL BE FRUITLESS.

X. *Extract from the Report refpecting Experiments made at the Polytechnic School in the Years V. and VI. on the Combuftion of the Diamond. By* C. GUYTON.

[Concluded from Page 61.]

Repetition of the Experiment in Fructidor, Year VI.

AFTER examining what means were moft proper for preventing the veffels from cracking by the inequality of their dilatation, it was found that there were none more certain than to employ, inftead of a glafs bell, a well-chofen globe of a moderate thicknefs, and of fuch a fize that its furface might be at a fufficient diftance from the point of the luminous cone.

The

The globe which we judged beſt for anſwering theſe conditions was 28·63 centimetres in diameter: its content was 123·25 decilitres, or 12,325 cubic centimetres, comprehending that portion of the neck which it was thought proper to retain, and which was 159 millimetres. That we might the more eaſily obſerve the riſing and falling of the mercury in the inſide, and thence determine the volumes of gas, we cemented on the outſide ſlips of paper, on which we traced, by means of meaſured veſſels, ſcales indicating decilitres, or or 100 cubic centimetres.

It may be readily conceived, that it was not poſſible to fill ſo frail a veſſel with mercury in order to diſplace it afterwards by oxygen gas. In transferring the gas by means of water, we ſhould have been obliged to leave a portion of that liquid on the interior ſide of the veſſel. We determined, therefore, to convey the gas, at the moment when it was diſengaged from the oxygenated muriat of pot-aſh, by means of a tube adapted to the diſtilling apparatus, and made to deſcend to the bottom of the globe in ſuch a manner that the common air ſhould be forced to iſſue from the globe by another pipe fixed in the ſtopper of the orifice, and communicating with the pneumatic tub.

This proceſs is exactly the inverſe of that which I propoſed in my work on aëroſtats *, to fill a balloon of inflexible matter with hydrogen gas. It was founded on the ſame principle—the difference of the ſpecific gravity of the two fluids. Here it had the advantage of leaving the veſſel perfectly clean; an important condition, and which it is ſo difficult to obtain when air is expelled by mercury.

It was readily foreſeen that the firſt portions of the oxygen gas would become mixed with the atmoſpheric air, and that it would be neceſſary to diſplace this mixture ſeveral times by new quantities of oxygen gas, that no more azotic gas might remain in it, or, at leaſt, that the remaining quantity ſhould be ſo ſmall as to be incapable of having a ſenſible effect on the reſults of the experiment. We had even contrived means to determine it, by receiving under the pneumatic bell the

* Deſcription de l'aëroſtat de Dijon, &c.

laſt portions diſplaced, that we might ſubject them to a eudiometric proof.

With this view we employed 18 decagrammes (about ſix ounces) of the oxygenated muriat of pot-aſh, which were put into a retort, at once to furniſh, at one operation, the whole quantity of the gas neceſſary for this renewal.

Thoſe who have not tried themſelves this method of ſubſtituting one aëriform fluid for another, might entertain ſome doubt reſpecting the purity of that employed in our experiment; but it will be eaſy for me to remove it. This was an article of ſo much importance that we could not neglect attempting to collect proofs of it.

We know with what ſucceſs M. Humboldt applied to the improvement of eudiometry. The intereſt which he took in our experiment induced me to invite him to come and determine himſelf with thoſe inſtruments, and by thoſe proceſſes, which were familiar to him, the purity of the oxygen gas in which the combuſtion was to take place. He readily accepted my invitation, as I had reaſon to expect, from his well known zeal for the progreſs of ſcience; and this article of our report is the production of his pen. I had likewiſe the ſatisfaction of ſeeing him apply eudiometric inſtruments to the examination of the reſiduum of the gas after the combuſtion. The following is the reſult of the proofs to which he ſubjected that introduced into the apparatus to ſerve for the combuſtion:—

Nitrous gas, diſengaged, by means of copper, from weak nitric acid, was tried with ſulphat of iron and oxygenated muriatic acid, which ſhewed in it from 0·09 to 0·10 of azot. One hundred parts of oxygen gas received, towards the middle of the operation, as it iſſued from the globe, in which we had propoſed to diſplace, in ſucceſſion, common air by oxygen gas, were mixed with 300 parts of this nitrous gas: there was an aëriform reſiduum of 0·66: making allowance for 0·27 or 0·30 of azot, pre-exiſting in the nitrous gas, we judged that, in 100 parts of gas tried, there were ſtill 36 of azotic gas.

We then continued to introduce into the globe freſh oxygen gas. We collected towards the end a portion which was

9 ſubjected

subjected to the same proof, in the same proportions, with the same nitrous gas. The residuum this time was only 30 parts; and as the 300 of nitrous gas employed contained nine or ten parts for $\frac{0}{0}$ of azot, we concluded that this oxygen gas was quite pure.—These are the words of M. Humboldt.

When this first condition was fulfilled, the question then was to place, in the centre of this globe, the diamond destined for the experiment. We had previously formed a small cup of the lower portion of a furnace-pipe, the tube of which, five centimetres in length, was fixed to an iron stalk, and this stalk was stuck into a cylinder of cork destined to be inserted in the neck of the balloon. This cork was dipped in mastic to shut its pores, and a small glass tube passed through it to establish a communication between the inside of the balloon and the mercurial tub. (See fig. 2. Plate II.)

We then placed the diamond on the cup, where we left it, having put it there by means of a ribbon, which we drew from under it in an instant, as soon as the balloon had been inverted, and its neck immersed in the mercury. The balloon in this position was made fast in a kind of collet, which rested on the edges of an iron mortar that served as a hydrargyro-pneumatic tub. We then exhausted, by suction, a portion of the oxygen gas sufficient to make the mercury rise to 12 centimetres above the orifice of the balloon.

The diamond was the same that had been already exposed to the action of the solar fire towards the end of the experiment of the preceding year, and which had lost only two deci-milligrammes, and consequently weighed 199.9 milligrammes (3·766 grains), or one carat wanting $\frac{1}{35}$ *.

On the 5th Fructidor last year, at one in the afternoon, we began to throw upon the diamond the focus of the large lens of the National Institute. The thermometer, exposed to the sun under a bell-glass, indicated 39·75: the mercury in the barometer stood at 75·89 centimetres, (28 inches 0·5 lines.) The volume of air, inclosed by means of the mercury in the tub, brought to the pressure of 757·7 millime-

* It is well known that the carat of the jewellers is only 205·72 milligrammes.

tres (28 inches), and to the mean temperature of 12·5 degrees of the decimal thermometer, according to the experiments of Cit. Pneier, and the tables of Cit. Prony*, was then found to be 11,470 cubic centimetres.

Having taken the neceſſary precautions to heat gradually the balloon, the point of the luminous cone being almoſt in the centre, we were obliged to cover with a glaſs plate the wooden ſupporter, which was already on fire. The diamond firſt exhibited a black point at the angle immediately ſtruck by the ſun. We afterwards ſaw it entirely black, and as it were charred: we diſtinctly perceived, a moment after, brilliant points in a ſtate of ebullition, as it were, on the black ground. The ſolar rays, having been for a moment intercepted, it appeared tranſparently red. The ſun becoming obſcured by a cloud, we ſaw it of a much purer white than it had been at the commencement of the operation.

The ſun having emerged from the cloud, the ſurface of the diamond aſſumed the appearance of metallic ſplendour: it was then ſenſibly diminiſhed, and there remained no more than a quarter, of a lengthened form, without angles or perceptible edges, but ſtill very white, and of a beautiful tranſparency. We obſerved a ſlight fiſſure at the bottom of the pipe which ſupported it, but without any ſeparation of the parts. I muſt not forget, that at the commencement of the combuſtion we thought we obſerved a purpureſcent cone ariſing from the ſupport in the pencil of the ſolar rays; but this phenomenon was only an optical effect, which depended on the poſition of the obſerver.

The whole apparatus was left in the ſame ſtate, only defended by an inverted box placed over it, until the 7th, when we again began, at one hour twenty minutes, to preſent the diamond to the focus. We ſoon obſerved the ſame phenomena as on the 5th, the black ſurface, the brilliant points in ebullition, which vaniſhed and re-appeared according to the intenſity of the focus: we ſaw alſo a brilliant metallic appearance, or rather leaden-colour. This is the expreſſion which the aſſiſtants employed to characteriſe this phenomenon. At one hour forty minutes the diamond was entirely

* Journal Polytechnique, Part II. p. 65.

conſumed.

consumed. We at first suspected that there still remained a brilliant particle; but we soon judged that it was a vitrified point of the support, which was confirmed on inspecting the pipe when drawn from the globe. The question now was to collect the products of the combustion. No means seemed likely to be attended with more certainty than to introduce water of barytes into the apparatus, taking care to adhere, as nearly as possible, to the proportions indicated for the saturation of the carbonic acid, which we supposed must have been formed to prevent the uncertainty which the excess of this re-agent might occasion in the results. The whole apparatus being removed into the shade, we began on the 9th to draw out the pipe which served as a support, and observed on it two slight fissures occasioned by the contraction, and a spot of four or five millimetres in diameter, the centre of which had a vitreous appearance, and its edges a reddish hue. On examining it with a magnifying glass, we observed at the lowest point a space of two or three millimetres diameter, the surface of which was really vitrified, but of a tarnished and unequal colour. We distinguished a particle of white glass, pure and brilliant, formed into a globule some smaller portions of the same nature, and two small globules of a vitreous substance, which had a greenish red colour.

On one side we observed on the edges several other very small globules interspersed in a yellowish ground, and on the opposite a slight tinge of very bright red with very small reddish points. A particle of white earthy matter was at first taken for a fragment detached from the edges of the pipe, but it was found friable, and ascertained, by the stain it left on gold, to be oxyd of mercury. This examination being finished, we introduced into the globe five measures of saturated water of barytes, each of 46·5 cubic centimetres. The liquor immediately assumed a milky appearance, and there was a diminution in the volume of gas, which, calculated by means of the attached paper scale, amounted nearly to 300 cubic centimetres. Thus we might stop here and consider the experiment as terminated, and by making some allowance for the errors unavoidable in such manipulations, and make the result tally with the amount, determined before by the noble experiment of Lavoisier and La Place, of the re-

ſpective quantities of carbon and oxygen which form the carbonic acid. Mr. Tennant ſeems to have done the ſame thing lately after the combuſtion of the diamond by nitre. But we ſhould only have confirmed what was before known, or ſuppoſed to be known. Our object was not only to obſerve, with more attention, what took place during the act of combuſtion, but to aſcertain, as accurately as poſſible, the nature and quantity of the product, and the reader will find that the labour undertaken on this ſubject has not been fruitleſs.

The liquor was agitated in the globe to mix the white matter which had been depoſited. We drew out 4¾ meaſures of the five we had introduced by making uſe of the ſame inverted bottle filled with mercury, and which we raiſed on the inſide by means of an iron ſtalk compoſed of ſeveral pieces, which could be adjuſted by ſcrews. (See fig. 6. Plate II.)

We introduced into the balloon three new meaſures, each containing the ſame quantity of diſtilled water, which was ſhaken in the inſide to detach and collect what adhered to the ſides. Theſe united liquors, being immediately filtered in an open filtre, left 192 centigrammes (36·142 grains) of carbonat of barytes dried in the heat of boiling water.

It may be readily judged what was our aſtoniſhment when proceeding to examine the liquor, inſtead of finding in it a ſlight exceſs of uncombined barytes, we obſerved that it changed neither the colour of turmeric nor that of logwood, and that, on the contrary, it acted on an infuſion of turnſole as water charged with the carbonic acid. The preſence of this acid unequivocally manifeſted itſelf, when we poured upon it a few drops more of barytes water, which immediately rendered it turbid. It was neceſſary to add even 4·65 centimetres of this water to ſaturate and precipitate the remaining acid gas. Being informed by this phenomenon that the production of the gas had been more conſiderable than we expected, and that ſome of it ſtill remained mixed in the aëriform fluid in the balloon, we took every meaſure neceſſary to determine the quantity. This we were luckily enabled to do by the diviſions which had been marked on the ſcales, the orifice of the globe having never been yet taken out of the mercury.

When the barytes water was taken out, the apparent volume

lume was found to be exactly 122 decilitres, the internal column of the mercury above the level of the tub was 47 millimetres; the barometer being at 759·96 millimetres, (Fructidor 19, year VI.) the centegrade thermometer at 21·25, the real volume, at a mean preſſure and temperature, was 112.426 decilitres, or 11242·66 cubic centimetres.

I ſtill invited M. Humboldt to co-operate with us in examining the nature of this reſiduum of gas. It was transferred in his preſence into a pneumatic tub prepared on purpoſe with diſtilled water, and received into four large flaſks. The trial was made by the ſame inſtruments, and with the ſame nitrous gas, which had ſerved for the oxygen gas before the combuſtion, and conſequently containing from 0·09 to 0·10 of azotic gas. The trials made on portions extracted from different flaſks varied from 31 to 34 in the quantity of the reſiduum of gas, in a mixture of 100 parts of gas examined with 300 parts of nitrous gas.

I ſhall not even take the mean term, I ſhall ſtop at the weakeſt, which indicates four hundredth parts of carbonic acid gas, which, I think, I can aſſert to be rather below than above the truth; ſince a portion of this ſame gas, brought into contact with ammonia, under a receiver, experienced a diminution of 4·5 *per cent.*

Let us now eſtimate the carbonic acid gas which entered into the compoſition of the 192 centigrammes of carbonat of barytes. According to Pelletier, whoſe accuracy is well known in reſearches of this kind, 100 of this earthy ſalt contain 22 of acid gas *, which gives 42·24 for 192; and as the cubic centimetre of gas weighs 1·847 milligrammes, it follows that the 42·24 centigrammes repreſent 228·621 cubic centimetres. If we now add, on the one hand, the 449 cubic centimetres, found in the reſiduum of the gas after combuſtion, and which, as we ſaw, formed the four hundredth parts; and deduct, on the other, the ſame quantity from the aëriform fluid in which the combuſtion was effected, it reſults, that in 11470 cubic centimetres of oxygen gas contained in the balloon, there remained, after the combuſtion, only 10793; that 677 were conſumed; that theſe 677 cubic centimetres of

* *Annales de Chimie*, Vol. XXI. p, 135.

oxygen

oxygen gas, in the ratio of 1·3577 milligrammes each, produced, with the 199·9 milligrammes of the diamond, 1117·96 milligrammes of carbonic acid.

In the last place, that, instead of the proportions 0·28 of combustible substance, and 0·72 of acidifying principle, observed in the combustion of carbon, the proportion was, for the combustion of the diamond, - 17·88 of carbon.
82·12 of oxygen.

100·00

Though it was not possible for me to doubt facts deduced from calculation, I at first hesitated to admit differences so considerable in the manner in which the same combustible united itself to oxygen in the quantities it could take up, and the products of its combustion; in a word, a carbonaceous combustible more abundant in real combustible matter than charcoal itself, and which at the same time differed so much from it in the degree of temperature necessary to determine the action of its affinity. But I soon began to reflect, 1st, That this would not be the only instance of the first degree of the oxydation of an acidifiable base having been operated with great difficulty, while the acidification was afterwards completed with the utmost facility: 2d, That several substances of the same kind presented to us also these two characters; a greater abundance in real carbon, and greater resistance to inflammation; so that they naturally placed themselves in an intermediary rank between the diamond and charcoal. These two considerations, still strengthened by the similarity of the phenomena observed during the course of our two experiments in the passage of the diamond to the state of carbonic acid, appeared to me to throw a ray of light on this subject hitherto so obscure.

In regard to the first consideration it will be sufficient for me to call to mind with what difficulty the commencement of a composition of azot and oxygen is formed by the direct way, and the high degree of temperature which it requires, while nitrous gas cannot be in contact with oxygen without passing immediately to the acid state. Charcoal will then be to the carbonic acid what nitrous gas is to the nitric, and the

the diamond will be to charcoal what azot is to nitrous gas. There will, therefore, be no longer occasion of wonder that more oxygen is necessary to that substance, which as yet has none of it, than to that which has already been united with the quantity necessary for arriving at the first point of saturation.

The second consideration rests on facts no less conclusive. Plumbago is a carbonaceous combustible, which does not burn but at a very high temperature, or in nitre in fusion; which produces by its combustion carbonic acid; which, as well as the diamond, is more abundant in combustible matter than carbon itself. We are indebted to the illustrious Scheele for the first observation of this fact. One part of carbon alcalises only five parts of nitre; one part of plumbago can alcalise ten. The operation performed in a retort on 80 centigrammes of plumbago, gave him 357 cubic centimetres of carbonic acid gas*. This agreement will not be contested by those who, having been witnesses of our experiment, so unanimously declared, that the surface of the diamond assumed instantaneously a leaden colour.

This mineral is not the only body which presents these striking characters of a substance almost incombustible, and yet very abundant, in combustible matter. I described, sixteen years ago, in the Memoirs of the Academy of Dijon, a fossil found in a mass in the coal-pits of the Rive-de-Gier, which was sent to me under the name of *incombustible coal*, and which I then considered as real coal which had passed to the state of plumbago. I characterised it in that manner.

Our brother Dolomieu has described a fossil of the same kind, which he calls *carbure of alumine*, which is the *anthracolite* of Werner. I had already suspected that it was neither the presence of four or five centiemes of alumine, nor that of a still smaller quantity of iron, that rendered it incombustible, but the little advanced state of the oxydation of the carbon. I subjected it to two experiments, by which this was fully confirmed.

The object of the first was to determine if the alumine present was in a state of combination sufficiently intimate to

* Mem. de Scheele, French edit. Vol. II. p. 27 and 29.

refift the action of pot-afh by the humid way: 100 parts, put in digeftion in that folvent, left in it 4·6 of alumine.

The fecond was, to afcertain whether this combuftible, which poffeffed fo little inflammability, had alfo the power to alcalife more nitre than carbon, confequently to take up more oxygen. Three fucceffive trials gave for a mean refult the alcalifation of 7·87 parts of nitre by one part of that mineral; and the fame coally matter, digefted for four or five days in oxygenated muriatic acid, burnt completely with 6·5 of nitre.

M. Klaproth, the celebrated chemift of Berlin, had before fubmitted to trials of the fame kind a foffil defcribed by M. Widenmann under the name of *incombuftible coal*, and found that 100 parts left, after combuftion at a very ftrong heat, only feven of a cineritious refiduum; that treated in a crucible with eight parts of nitre, and the mafs diffolved in water, acids occafioned no precipitate. Kirwan, in his experiments on coal, remarks, that that which he calls *Kilkenny coal*, and which has a metallic brilliancy, which does not burn but when carried to incandefcence, and which then comfumes flowly without emitting flame, can decompofe 9·6 of nitre.

After this I do not fee how there can remain any doubt that thefe fuppofed incombuftible fubftances are real oxydes of carbon, which, like coal or charcoal, have the property of conducting the electric fluid; of cementing iron; of taking the oxygen from fome acidifiable bafes; but which are not at that degree of oxydation neceffary for exercifing this feparating affinity at a weak temperature.

I muft not omit this opportunity of making fome application of this principle, which may become ufeful to the arts. It has not yet been fufficiently explained, why fome animal and vegetable matters produce carbon fo difficult to be incinerated; why charred pit-coal, known under the name of coke, or cinders, and which has been half burnt in the preparation, is, however, fo powerful a combuftible; why peat, or turf, the weakeft of combuftibles, acquires, by being properly charred, the property of welding large pieces of iron better than charcoal; and why, in the laft place, charcoal, when expofed to a very ftrong heat in veffels impenetrable

to

tо air, becomes there, in a certain degree, incombuſtible, as is proved in the experiment made by Mr. Tennant, which I have mentioned in the article Air in the *Dictionnaire de Chemie Encyclopedique*, Vol. I. p. 714.

The anſwer to all theſe queſtions may be found in the theory I have laid down: they are charcoals in the firſt degree of oxydation. Thus ſome of them have not yet acquired that which conſtitutes charcoal properly ſo called; others, after poſſeſſing all the qualities of vegetable and mineral carbon, have returned to the firſt degree by a real (*debrulement*) unburning of the remaining carbon; ſo that by loſing their inflammability they become capable of fixing a greater quantity of oxygen, and conſequently of ſetting at liberty a greater quantity of caloric when they find themſelves at a temperature ſufficiently high to determine and complete their acidification.

Some practical conſequences will doubtleſs hence be deduced in regard to proceſſes for the reduction of metals; for the cementation of ſteel, which it is probable takes up only oxyd of carbon, ſince it is ſeparated from it in that ſtate; for the incineration of the carbonaceous reſiduums of our analyſes; for the carboniſation of wood, pit-coal, and turf: in a word, we may perhaps thence conclude the poſſibility of rendering uſeful thoſe maſſes of pit-coal, ſaid to be incombuſtible, found at Rive-de-Gier, by mixing it with more inflammable matters, to maintain the temperature which determines its combuſtion. Its poſition, texture, and all its exterior characters announce, as already ſaid, that it conſiſts of beds of coal changed by a ſubterranean fire; and this is confirmed by tradition, which preſerved to that mountain, for three centuries, the name of the *Mountain of Fire.* (*Montagne de Feu.*) We can now pronounce that it is coke too far advanced, but ſo much the more ſuſceptible of producing a great heat under favourable circumſtances.

RECAPITULATION.

I ſhall here enumerate the conſequences, or rather the facts, which reſult from the phenomena obſerved in the two

combuſtions of the diamond by the ſolar fire, and the experiments which followed:—

1. It is not only by the colour, weight, hardneſs, tranſparency, and other ſenſible characters, that the diamond *differs* from charcoal, as ſeems hitherto to have been believed;

2. Nor is it by the ſtate alone of the aggregation of the matter that conſtitutes diamond:

3. Neither is it on account of the 200th part of the cineritious reſiduum left by carbon, or the ſmall quantity of hydrogen which it contains.

4. It is more eſſentially by its chemical properties that it differs.

5. The diamond is the pure combuſtible ſubſtance of this genus.

6. The product of its combuſtion, or of its combination with oxygen to ſaturation, is carbonic acid without reſidue.

7. Carbon burns at a temperature eſtimated at 188° of the centigrade thermometer; the diamond does not inflame but at about 30 pyrometric degrees, which, according to Wedgewood's ſcale, makes a difference of 188 to 2765.

8. Charcoal, when kindled, maintains of itſelf, in oxygen gas, the temperature neceſſary for its combuſtion. The combuſtion of the diamond ſtops when you ceaſe to maintain it by a furnace-heat, or the union of the ſolar rays.

9. The diamond, for its complete combuſtion, requires a much greater quantity of oxygen than charcoal does, and produces alſo more carbonic acid. One part of charcoal abſorbs in this operation 2·527 of oxygen, and produces 3·575 of carbonic acid. One of diamond abſorbs a little more than four of oxygen, and really produces five of carbonic acid.

10. There are ſubſtances which are in a ſtate of intermediary compoſition between the diamond and charcoal. Theſe are plumbago, or native carburet of iron; incombuſtible foſſil coal; the carburet of alumine of Dolomieu; the anthracolite of Werner; the black matter united to iron in the ſtate of caſt iron and ſteel; carbonaceous reſiduums difficult to be incinerated; and carbon itſelf unburnt, (*débrulé*,) by the action of a ſtrong heat without the contact of air.

 11. Theſe

11. Theſe ſubſtances mixed, or weakly combined with three or four hundredths of their weight of iron, or alumine, give by their combuſtion carbonic acid, like charcoal and the diamond.

They approach to carbon by their colour, their lightneſs, their opacity; by their ſerving, like it, to decompoſe water, to cement iron, to deoxydate metals, to deoxygenate ſulphur, phoſphorus, and arſenic; and by conducting, like it, the electric fluid. They approach the diamond by containing more combuſtible matter than charcoal; by abſorbing alſo more oxygen, and producing more carbonic acid; by decompoſing more nitrous acid; by burning only at a much higher temperature, even in nitre in fuſion; and by their combuſtion being ſtopped when this temperature is lowered. They ſeem to differ from each other by the property of producing with zinc galvanic irritation, as well as ſilver does: which can be effected neither by the diamond nor charcoal.

12. Thus the diamond is pure carbon, the pure acidifiable baſe of the carbonic acid. Its combuſtion is effected in three periods, which require three different temperatures. At the firſt, which is the higheſt, the diamond aſſumes a black leaden colour. It is an oxydation in the firſt degree, the ſtate of plumbago and anthracolite. At the ſecond temperature, which may be eſtimated at 18 or 20 pyrometric degrees, there is a ſecond ſlow and ſucceſſive combination of oxygen. It is a progreſs of oxydation which conſtitutes the habitual ſtate of charcoal, or rather that in which it is found after the action of a ſtrong heat in cloſe veſſels has diſengaged a part of its oxygen.

Thus plumbago is an oxyd of the firſt degree, charcoal an oxyd in the ſecond, and the carbonic acid the product of the complete oxygenation of the carbon.

Suppoſing, then, that we operate with ſufficient preciſion to take away from the ſurface of the diamond the black matter in proportion as it is formed, by ſuddenly withdrawing from it each time the action of the ſolar fire, we ſhould doubtleſs be able to convert it into charcoal, or at leaſt plumbago, if the too rapid paſſage of the laſt degree of oxydation to oxygenation did not prevent us from ſurpriſing it in that ſtate.

 13. In

13. In the laſt place, ſeveral conſequences, of importance to chemiſtry and the arts, ariſe from theſe principles.

After this concluſion it will be aſked, no doubt, how it happens that the ſimple matter, pure carbon, the diamond, is ſo rare while its compounds in different ſtates are ſo abundantly diffuſed? To put an end to the aſtoniſhment of thoſe who might entertain any miſtruſt, I ſhall obſerve, that aluminous earth is alſo one of the moſt common matters, and that adamantine ſpar, as rare as the diamond, is however only alumine; that iron every where exiſts, under all forms, except in the ſtate of purity: the exiſtence of native iron is ſtill doubtful. The wonder exiſts only in the oppoſition of facts to our opinions, and will diſappear in proportion as we diſcover, and appropriate to ourſelves, the means employed by nature in producing the ſame effects.

Thoſe who have never turned their attention to the phyſical ſciences, to eſtimate at leaſt their influence on public felicity, are diſpoſed to treat as vain curioſity labours which are not immediately directed towards a near object of new enjoyment. What would have been their aſtoniſhment had they been told, that reſearches on the nature of the diamond would one day produce truths which might give riſe to happy changes in the practice of the moſt familiar arts; in the preparation, and in the employment of the coarſeſt combuſtibles! Such, however, are the conſequences that may ariſe from the beſt known properties of the eſſential principle carbon in its different ſtates.

XI. *A Letter to the Editor, containing ſome Objections to the Mitchillian Theory of Peſtilential Fluids.*

SIR,

I OBSERVE, in the 15th Number of your uſeful Magazine, a communication from Dr. Mitchill, of New-York, ſhewing the utility of conſtructing the houſes, and paving the ſtreets of cities, with *calcareous* in preference to *ſiliceous* and *argillaceous* materials. This communication is contained in a letter from Mr. Da Coſta to the Doctor, in which

it

it is obferved, that although the city of Lifbon is one of the filthieft in Europe, and the moft infefted with putrid effluvia, yet that it neverthelefs is remarkable for its falubrity. This is attributed by Mr. Da Cofta to the calcareous materials, of which the buildings and pavements are conftructed, abforbing and neutralifing the feptic fluids, the caufe, according to Dr. Mitchill's theory, of all contagion.

In reply to this I would obferve, that from the author's own account it is evident that the calcareous earth in the city of Lifbon does *not* deftroy the putrid exhalations in any fenfible degree, and therefore cannot prevent any diforders to which they may give rife. The houfes, he obferves, are very offenfive, from the privies, and from inattention to cleanlinefs. The mouths of the fewers go to the wharves, and are bare at low water. The fluid which comes from thefe contains fo many infectious matters that its *ftrong putrid fmell can fcarcely be endured!* It is plain, therefore, that the putrid exhalations are not deftroyed, or, at leaft, that they are fo quickly generated as to annoy the olfactory fenfe in an intolerable degree. The bodies of the inhabitants are conftantly immerfed in putrid exhalations, yet contagion is not produced. The inference is, that putrid exhalations are not its immediate caufe. Contagion arifes where no putrefaction is going on, at leaft in any degree perceptible to the fenfes; as when a number of perfons are crowded together in jails, and even in the houfes of the poor in the winter feafon, without fufficient ventilation. On the other hand, putrefaction may be prefent, in a very high degree, without giving birth to contagion, as may be obferved in flaughterhoufes, cat-gut manufactories, &c.

But, is it proved fufficiently that calcareous matters do abforb and neutralife feptic exhalations? Mr. Da Cofta remarks, that he has obferved, two or three times, in Lifbon, dead animals upon the ruins of houfes, and of courfe furrounded by calcareous earth, in a ftate of deficcation; and, at the fame time, two or three fathoms diftant, another animal dead too, and lying upon another kind of foil, in a ftate of complete putrefaction. I do not prefume to queftion the truth of this, fince it was a matter obvious to the fenfes: but I deny

I deny the generality of the fact equally from obfervation. I fufpended a piece of flefh, in a proper veffel, over a layer of chalk, and at the fame time laid chalk, in fmall pieces, on gauze, a few inches above the flefh: but I did not obferve that putrefaction was more backward than in other circumftances, or that putrid exhalations were lefs extenfively diffufed around. That quicklime deftroys putrefaction, is no proof of the opinion here advanced, fince this can be accounted for on other principles.

If fevers abound on the oppofite fide of the Tagus, whilft the city of Lifbon is free from them, there are probably other caufes to which this may be afcribed, fuch as a marfhy foil, &c. Whether any thing of this fort exifts, the account here given by Mr. Da Cofta does not enable us to judge.

It is the tendency of Dr. Mitchill's theory to overturn fettled opinions, and to caufe us to abandon practices, which have been generally employed, and much relied on, for the deftruction of contagious matter. It is of great importance, therefore, that it fhould be well founded, left we be induced to lay afide means that are efficacious on grounds not fufficiently eftablifhed. Whatever becomes of Dr. Mitchill's hypothefis, it appears to me to receive no fupport from the facts and arguments of the paper in queftion, although it is confidered by him as affording a ftrong confirmation of its truth.

Wifhing every fuccefs to your valuable publication, I remain, Sir, yours, &c.

H. CLUTTERBUCK.

Walbrook, Oct. 21, 1799.

XII. *Defcription of Mr.* Howard's *Improved Air-Furnace.*

THE difficulty of obtaining a degree of heat fufficiently intenfe for many operations in chemiftry, has been felt and lamented by every one engaged in fuch purfuits; and, notwithftanding the refearches and numberlefs experiments made profeffedly for that purpofe by the greateft men, the beft conftruction of an air-furnace is ftill a problem; indeed,

deed, the rules laid down by thoſe who have written on this ſubject, differ ſo widely from each other, that we muſt ſuppoſe a great number of circumſtances, hitherto overlooked, enter into the conſtruction of a good furnace. Having obſerved effects produced in that of Mr. Edward Howard, of Doughty-ſtreet, which we have ſeldom, if ever, ſeen equalled in any other furnace, we thought we could not better oblige our chemical readers than by giving a deſcription of it. The difference in the preſent compoſition of Wedgewood's pyrometer pieces, from that uſed by him when he firſt invented his inſtrument, makes it impracticable (unleſs the ſcale was alſo altered) to meaſure correctly the degree of heat obtained; it muſt however be at leaſt = 160°, and is ſufficient to run down the beſt Heſſian crucibles. The moſt ſtriking deviation from the common conſtruction is in the lower part of the chimney, or flue, being ſmaller than the upper, and the greater depth of the aſh-pit. Mr. Howard thinks that ſomething depends on the direction of the horizontal funnel, which in this caſe opens to the north. If any of our readers ſhould conſtruct a furnace on this plan, we ſhould be glad to be informed of the coincidence of effect*.

Fig. 1. (Plate IV.) A, the cavity or body of the furnace, 9 inches ſquare and 1 foot 7 inches deep to the bars. B, the aſh-pit, 1 foot 2 inches, by 9 inches broad, and 2 feet 8 inches deep below the bars: this aſh-pit opens to the external air by an horizontal funnel C, 6 feet long, paſſing under the floor of the laboratory: this not only furniſhes a ſupply of denſer air, but prevents the unpleaſant effects of the cold draught on the legs and feet of the operator, which happens when the aſh-pit opens into the laboratory. The external opening of this funnel is about 2 feet ſquare, which gradually contracts to the aſh-pit in the manner ſhewn in the plate. D, the aperture of the horizontal flue, 7 inches wide by $2\frac{1}{2}$ deep, contracting in width to $4\frac{1}{2}$ inches, where it enters the vertical flue or chimney, which is $4\frac{1}{2}$ inches ſquare, of which width it continues to the height of 7 feet, and is then enlarged to 5 inches ſquare; which dimenſions it preſerves to

* Dr. Pearſon's furnace, which is a very powerful one, agrees in ſome parts of its conſtruction with Mr. Howard's.

the

the top of the brick-work, being $16\frac{1}{2}$ feet from E. The chimney finifhes by an iron pipe about 3 feet long.

Fig. 2, F, fhews the plan of the body of the furnace 9 inches thick, the top courfe of bricks being bound together in the ufual manner by a ftrong iron hoop G. H is an horizontal fection of the chimney and flues *. When only a moderate degree of heat is wanted, the horizontal draught-hole C can be clofed by a fliding door or regifter, and an opening made into the afh-pit, directly under the bars, by removing the ftone ftopper I. (Fig. 1 and 2) This alfo is ufeful for occafionally clearing the bars from fcoria, or entirely removing them, in order fuddenly to put out the fire. The whole of the furnace and chimney is built of Windfor bricks.

XIII. *An Account of Mr.* Brown's *Travels through Egypt and Syria*, &c.

[Concluded from Page 76.]

MR. BROWN retired a fecond time to Cobbé, with little hopes of ever leaving the country. Of the property, which the king's agents had purchafed on his arrival, no part of the price had yet been paid. He had been infulted with the mockery of juftice, yet obliged to thank his oppreffors for the compenfation which their corruption and malignity alone had rendered incomplete.

He had not omitted to renew to the Melek Mufa the requeft which had been previoufly made to Mifellim and Ibrahim. He explained to him, in the leaft exceptionable manner, his intention of coming to Cobbé; completely did away all the fufpicions which his enemies had at firft excited; and concluded with defiring permiffion to go to Sennaar, or to accompany the firft *felatea* (an armed expedition for the purpofe of acquiring flaves,) to the fouth or fouth-weft; or to have a fafe conduct, and one of the Sultan's flaves, to accompany him to Bergoo, (the firft Mahometan kingdom on the weft.) By the firft route he hoped to have reached Abyffinia, or, if that had been impracticable, to have gone through Nubia to Egypt, or by Suakim to the Red Sea, and thence

* The chimney is detached about an inch from the wall behind all the way to the top.

to

to Mocha or Jidda. By the fecond he was almoft certain of fettling fome important points relative to the White River, poffibly of tracing it to its fource. And by the third, either of paffing directly weft, and tracing the courfe of the Niger, or of penetrating through Bornou and Fezzan to Tripoli.

To the firft propofal he anfwered, in a manner which gave Mr. Brown reafon to doubt his fincerity, that the road to Sennaar was unpaffable, the Sultan being then mafter of only one half of Kordofân: that the natives of all that part of it which remained unfubdued were his implacable foes, and would infallibly deftroy any perfon who came from Dar-Fûr: that he thought, however, if Mr. Brown waited another year, that route might poffibly be more fecure; and in cafe it fhould, he would ufe all his efforts to obtain the Sultan's permiffion for his departure.—Of the *Selatea* he faid, that our traveller would only encounter certain death by attempting it, as, between the jealoufy of thofe who accompanied him, and the actual hoftility of the country, there would be no hope of efcaping. Mr. Brown hinted, that the Sultan might give him a few attendants, whom he was very ready to pay, and an order to enable him to pafs unmolefted as his phyfician in fearch of herbs. He replied, that he would propofe fuch a meafure, but he did not expect it would receive the Sultan's approbation.—To the third propofal he anfwered, that he had no hope of Mr. Brown's fucceeding, and concluded with ftrongly recommending to him to feize the firft opportunity of returning to Egypt; but he affured him, if he could accomplifh any of the meafures he fo much wifhed, he would not fail to inform him and to afford him the neceffary aid. Such was the ftate of affairs when Mr. Brown returned to Cobbé, dejected, and with little expectation of realifing even his leaft fanguine hopes. Not more than fix weeks after this converfation had taken place, he was fent for to attend the Melek, who was confined by an old diforder in his lungs. He found him yet fenfible; but his eyes were fixed, and extremities incapable of motion. In five hours after, he expired. Thus were blafted our traveller's returning hopes of fuccefs; for no mediator now remained between him and the monarch, and no longer was there

near the court a man even of feeming liberality and good fenfe, to whom his projects might be fafely opened.

During the fummer of 1794, five men who had exercifed confiderable authority in fome of the provinces, were brought to El Fafher as prifoners; it was faid that they had been detected in a treafonable correfpondence with the hoftile leader, Hafhem, in Kordofân. They did not undergo any form of trial; but as the Sultan chofe to give credit to the depofitions made againft them, his command was iffued for their execution. Three of them were very young men, the youngeft not appearing to be more than feventeen years of age. A little after noon they were brought, chained and fettered, into the market-place, before one of the entrances of the palace, efcorted by a few of the royal flaves armed with fpears. Several of the Meleks, by the monarch's exprefs order, were prefent, to witnefs, as he termed it, what they might expect to fuffer if they failed in their fidelity. The executioner allowed them time only to utter a fhort prayer, when he plunged his knife into the neck of the oldeft, exactly in the fame manner as they kill a fheep. The operation, too, is marked by the fame term, *dhebbah*. He fell, and ftruggled for fome time. The reft fuffered in their turn. The three laft were much agitated, and the youngeft wept. The two firft had borne their fate with becoming firmnefs. The crowd that had affembled had fcarcely fatiated itfelf with the fpectacle of their convulfive motions while proftrate in the duft, when the flaves of the executioner coolly brought a fmall block of wood and began mangling their feet with an axe. Having cut off their feet, they carried away the fetters which had been worn by the criminals, though of little value, and left the bodies where they were. Private humanity, and not public order, afforded them fepulture.

Near the end of the year 1795 a body of troops was muftered and reviewed, intended to replace thofe who had died of the fmall-pox in Kordofân, which, it was faid, amounted to more than half the army. The fpoils which had been taken from Hafhem were alfo oftentatioufly difplayed on this occafion. They confifted of eighty flaves, male and female, but the greater proportion of the latter, many of whom were

exceedingly

exceedingly beautiful, nor the lefs interefting, that, though the change in their fituation could not be very important, their countenances were marked with defpondency. To thefe fucceeded five hundred oxen and two hundred large camels; the whole proceffion was clofed by eighty horfes, and many articles of lefs value borne by flaves. Shouts rent the air of, Long live El Sultan Abd-el-rachmân el rafhîd! May God render him always victorious!

A fhort time after, Mr. Brown caufed a petition to be drawn up, which was prefented by Ali-el-Chatîb to the Sultan, in which he ftated his fufferings, requefted payment of what yet remained due to him, and permiffion to proceed on his journey to Kordofân. Though the perfon who prefented it was a man of confiderable weight, no anfwer was given. He therefore followed it up by a vifit in perfon, which he had refolved fhould be his laft. His arrival was no fooner known than he was directed to attend fome fick perfon, as he had feveral times done before. This he pofitively refufed, and it was many days before he could be admitted at court. On the 11th of December 1795, however, he accompanied the Chatîb to the monarch's prefence, and briefly ftated what he came to requeft; which the former feconded, though not with the zeal Mr. Brown wifhed. To his demand of permiffion to travel, no anfwer was returned. But the generous and hofpitable monarch, who had received from him the value of 750 piaftres in goods, though his claim was well fupported, gave him only twenty meagre oxen, in value about 120 piaftres! The ftate of his purfe would not permit him to refufe even this mean fupply; and he bade adieu to El Fafher, as he hoped, for ever.

Having applied the value of the oxen to preparations for his journey to Egypt, the report of the caravan's departure growing daily ftronger, he loft no time in joining the Chabîr, who was then encamped at Le Haimer, (3d March 1796,) a fmall village about three days journey north of Cobbé, where there was a tolerable fupply of water, but no other requifite for living.

Mr. Brown arrived at Le Haimer about a month before Ramadan; and it was not till the fixth day of El Hedge, the

 fecond

ſecond month after that faſt, that they actually commenced their journey to Egypt. In the mean time, having pitched a tent under a great tree, where they were ſheltered from the rays of the ſun, he fed on polenta (*as-eid*) and water with the camel-drivers. He had collected eight camels for the journey, but the beſt of them was ſtolen while grazing; another died; and to ſupply his place he was obliged to ſeek one on credit, for his whole exchangeable property at that time amounted only to eight piaſtres.

The journey, once commenced, was continued with little remarkable except violent heat. They returned by the only caravan route, *Bir el Malah*, *Legbea*, *Selime*, *Sheb*, and *El-wah*. Their proviſions were indifferent, and in ſmall quantity. The camel-drivers regaled themſelves with the fleſh of theſe animals when they chanced to be diſabled on the road. When they arrived at *Beiris*, they were met by a Caſhef, who welcomed the Jelabs with an exhibition of fire-works. On this occaſion it is cuſtomary to treat the chief merchants with coffee, and to preſent to each a *beniſh* of coarſe cloth, worth about a guinea; but he expects in return a ſlave from each, worth about ten guineas. When Mr. Brown arrived at Aſſiût, he had been four months without eating animal food; and the hard living, heat, and fatigue, had brought on a diarrhœa, by which he was much weakened; but before he left Aſſiût, where he paſſed twenty days, it was much abated.

After ſome ſtay in Egypt Mr. Brown embarked for Syria on the 19th of January 1797; and having viſited Jeruſalem, Damaſcus, Aleppo, and various other places, proceeded to Conſtantinople; from which he returned to England by Vienna, Dreſden, Leipſic, and Hamburgh. He arrived in London on the 16th of September 1798, after an abſence of nearly ſeven years.

XIV. *Tenth Communication from* Dr. THORNTON, *Phyſician to the General Diſpenſary*, &c. &c. &c. *relative to Pneumatic Medicine.*

ONE of the moſt dreadful of human afflictions is that where water oppreſſes the brain; and there are no hopes of any

any medicine getting to that part, except what firſt can enter the blood. Mercury has been ſucceſsfully employed; but it produces ſo great a derangement of the general health, that a ſubſtitute is certainly adviſable. Can the abſorbents of the brain be equally excited by vital air?

Lydia Johnſon, æt. 13, living at No. 5, Huſband-ſtreet, after a putrid fever became perfectly blind, and had frequent fits, evidently ariſing from oppreſſion of the brain. Various means had been employed without the leaſt advantage. I referred her to Meſſrs. Wathen and Phipps, that they might ſee the caſe. Under the inhalation of vital air, with the aid of bark and ſteel, the fits ſoon gave way; and afterward, finding the air produced no miſchief, I increaſed the quantity one day to twelve quarts, mixed with three times that quantity of atmoſpheric, and almoſt immediately after the inhalation ſhe had a dawning of ſight, and going home ſhe did nothing but cry out, Mama, mama! I ſee now every thing. The cure has remained permanent above a year, and I ſaw her the other day in perfect health and ſpirits.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

ROYAL SOCIETY OF LONDON.

IN the courſe of the preſent month (November) the Second Part of the Tranſactions for the year 1799 was delivered to the Members. The contents are:—An account of the diſſection of an hermaphrodite dog: to which are prefixed, ſome obſervations on hermaphrodites in general. By Everard Home, Eſq. F. R. S.—An enquiry concerning the weight aſcribed to heat. By Benjamin Count Rumford, F.R.S.M.R.I.A. &c. —An account of ſome experiments on the fecundation of vegetables: in a letter from Thomas Andrew Knight, Eſq. to the

the Right Hon. Sir Joseph Banks, K.B. P.R.S.—Observations on the different species of Asiatic elephants, and their mode of dentition. By John Corse, Esq. Communicated by the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—Some observations on the structure of the teeth of graminivorous quadrupeds, particularly those of the elephant and sus Æthiopicus. By Everard Home, Esq. F.R.S.—Experiments to determine the quantity of tanning principle and gallic acid contained in the bark of various trees. By George Biggin, Esq. Communicated by the Right Hon. Sir Joseph Banks, K.B.P.R.S.—Essay on the resolution of Algebraic equations; attempting to distinguish particularly the real principle of every method, and the true causes of the limitations to which it is subject. By Griffin Wilson, Esq. Communicated by Edward Whitaker Gray, M.D. Sec. R.S.—On different sorts of lime used in agriculture. By Smithson Tennant, Esq. F.R.S.—Experiments and observations on shell and bone. By Charles Hatchett, Esq. F.R.S.—A catalogue of Oriental manuscripts, presented to the Royal Society by Sir William and Lady Jones. By Charles Wilkins, Esq. F.R.S.—Presents received from November 1798 to June 1799.

On the 7th instant the Society met for the first time since the long vacation, when the Croonian Lecture on muscular motion, by Everard Home, Esq. was read. The subject was on the construction of the membrana typani. At the same sitting a picture, by Mr. Brown, of the late Mr. Smeaton, civil engineer, was presented to the Society by Alexander Aubert, Esq.*

Nov. 14. The conclusion of the Croonian Lecture was read, and a letter addressed to Count Bruhl on the orbit of the comet seen in August.

Nov. 21. A paper by Dr. Herschel on the power of penetrating into space by means of telescopes. This, the Doctor remarks, is connected with something different from their magnifying power. The extreme sensibility of the optic

* The meeting-room is hung round with portraits of deceased members; that of Sir Isaac Newton being placed over the president's chair.

nerve

nerve is ſuch as to require twenty minutes reſt, on coming from the light, before (when his head is covered with a black hood) he is able to diſcern ſmall teleſcopic ſtars: an equal time is neceſſary after a ſtar of the ſecond or third magnitude paſſes the field of the teleſcope.—The paper was not gone through at this ſitting.

METEORS.

Fiery Meteors have been more than uſually common of late.

On Saturday night, the 2d of November, at half paſt ten o'clock, a meteor, or ball of fire, paſſed through the air immediately over the town of Pocklington, accompanied with a moſt beautiful train of fire reſembling the tail of a rocket, and appearing to be about 15 yards in length. The direction was from the north-eaſt to the ſouth or ſouth-weſt, and continued viſible for about half a minute.

On Tueſday morning the 12th of November, about ſix o'clock, a meteor, or ball of fire, accompanied with a beautiful train, was obſerved in different parts of Staffordſhire. It was preceded by ſeveral flaſhes of vivid lightning.

The ſame, or a ſimilar meteor, was ſeen at the ſame time in Yorkſhire. The following is a deſcription of it, as ſeen at Hull and the neighbourhood:—On the morning of the 12th of November, between the hours of five and ſix, the heavens exhibited an awfully grand appearance. The ſetting moon became partially obſcured by dark cloudy ſpots or ſtreaks: in oppoſition to her was ſeen a lunar rainbow of the moſt beautifully varied colours; after which, the middle region of the air was illuminated by meteors croſſing each other in different directions, and leaving behind them long ſparkling trains, which were viſible for two or three minutes after theſe luminous bodies had diſappeared; one of theſe meteors, more brilliant than the reſt, illuminated the whole firmament, and, by its apparent approximation to the earth, created ſome alarm. The thermometer was that morning at 50 degrees. The air, which the preceding night was cold and froſty, became remarkably cloſe and warm, and produced on the walls

walls and furniture in houſes an unuſual dampneſs and humidity.

CONVERSION OF IRON INTO STEEL.

In our laſt we noticed that Mr. Muſhet, in varying the experiments ſuggeſted by C. Clouet's proceſs for the converſion of iron into ſteel by cementation with carbonat of lime, had found that when lime, *previouſly deprived of its carbonic acid*, was uſed with the iron, the reſult was, notwithſtanding, caſt ſteel; and that, therefore, he found himſelf obliged to reject the idea of the carbon neceſſary to the converſion of iron into ſteel, having been furniſhed by the decompoſition of carbonic acid.

We alſo in our laſt Number laid before our readers C. Guyton's report of the converſion of iron into caſt ſteel by means of the diamond, which is generally held to be pure carbon, and which, having diſappeared in the proceſs, was therefore believed to have entered into chemical union with the iron by the affinity excited between them by means of an high temperature.

From the tenor of the letter from Mr. Muſhet, which furniſhed us with matter for the notice firſt above referred to, we were led to ſuſpect that, however probable it might be that in the caſe laſt mentioned the carbon, which went to ſteeliſy the iron, was furniſhed by the diamond; yet, as Mr. Muſhet's experiments went to ſhew that carbon could find its way from the ignited gas of the furnace to the iron, through materials which, on firſt view, one would hardly think pervious to ſuch a principle, it was certainly poſſible that the French chemiſts might be miſtaken, and that the carbon, which converted the iron, might not after all have been furniſhed by the diamond.

Since, then, we correſponded with Mr. Muſhet on the ſubject, and propoſed that the experiment made at the Polytechnic School ſhould be repeated, only *keeping out the diamond*. As the idea aroſe from, and might indeed be ſaid to be embraced in, the facts preſented by Mr. Muſhet; and as the proceſs coincided ſo nearly with others which he had communicated as executed and in train, we knew no perſon

ſo likely to do it complete juſtice, or ſo well qualified by great experience to guard againſt error. In this we have not been deceived. Mr. Muſhet, whoſe zeal in every thing that concerns the improvement of the Britiſh iron manufactory entitles him to the gratitude of his country, has proved, by ſeveral concluſive experiments, that the French chemiſts would have had ſteel from their experiment even if no diamond had been employed in it. We ſhall not detain our readers longer, but content ourſelves with laying before them

An Account of the Experiments made by Mr. Muſhet with a view to prove whether the Experiment made at the Polytechnic School reſpecting the Converſion of Iron into Seel by means of the Diamond is concluſive.

I. " I introduced into a crucible ſome pieces of ſoft malleable iron weighing 1250 grains. A larger crucible was next taken, half filled with ſand pounded from the fireſtone of which the blaſt-furnace hearths at Clyde are made. The ſmall crucible containing the iron was inverted upon the ſurface of the ſand in the ſecond crucible, and forced ſo far into it as to bring the iron and ſand nearly into contact. The ſpace betwixt the exterior ſurface of the ſmall crucible, and the interior ſurface of the large one, was completely filled with the ſame ſand; the bottom of the inverted one alſo received half an inch of ſand. This quite filled the outer crucible, and made it have the appearance of being entirely filled with ſand: a well fitted fire-clay cover ſealed the whole. After a violent heat for 65 minutes the crucibles were withdrawn from the furnace: when cool, I found a ſlight crack in the exterior one; the ſand however remained entire, and firmly cemented together, though not vitrified, forming a thick porous lute ſurrounding the ſmall crucible. Upon removing the ſand, and examining the interior veſſel, I found the pieces of iron fuſed into one ſolid maſs, which proved ſoft ſteel. The maſs of ſand, which occupied the mouth of the inverted pot, contained ſeveral detached globules of ſteel; theſe, during the intenſity of the heat, and the extreme diviſion of the fluid, had penetrated the ſand, and one of them had actually reached the bottom of the outer crucible. Theſe maſſes

 had

had in their descent conveyed a blackish-blue colour to the sand, and the fusion of the whole had glazed the bottom and sides of the inverted crucible, unoccupied by the sand, with the same colour. The principle button, and small pieces, weighed 1229 grains: loss, 21 grains, $= \frac{1}{60}$ of the original weight of the iron.

"The quality of this steel was uncommonly red-short, and it was with difficulty it would take a form under the hammer. When cold it was much tougher than good steel is ever found to be, and distended in this state astonishingly under the hammer. The fusion, however, had been most complete; every part of the result possessed a crystallised surface more or less accurate. Upon the whole, the quality of this steel is, I think, similar to some described lately by C. Clouet. Its uncommon softness, while it possessed not the property of drawing into shape with a smooth uncracked surface, led me to conceive that the fusion was urged too speedily, and before the sufficient quantity of carbon had been taken up which constitutes good steel. This induced me to repeat the experiment in the following manner:—

II. "Having prepared, in an annealing-furnace, three crucibles, made from Sturbridge clay, of various sizes; into the smallest one I put five pieces of malleable iron weighing 1875 grains; upon the top of which I wedged a high-baked cover of fire-clay: upon the top of this cover, and to a level with the edges of the crucible, I introduced infusible sand. This operation was performed upon a plate of red-hot iron, lest the sudden contact of the cold air should endanger the soundness of the pots. The small crucible was next introduced into the second size, and a cover fitted over the two. Lastly, this double crucible was placed into one so much larger as to admit of a stratum of sand, nearly half an inch thick, in such a manner as to insulate the former most completely. This operation finished by covering the outside crucible with a large fire-clay cover. My reason for leaving the vacant space betwixt the two innermost crucibles not filled up was, to guard against any error in the result, which might probably be occasioned by the action of the interior fire-clay cover, and the sand, in their respective degrees of

shrinkage and expansion. The whole was exposed to a moderate white heat for nearly an hour, and for forty minutes further a heat equal to the whole power of the furnace. The crucibles were safely withdrawn, and, when examined, the following facts presented themselves:—

"The outer crucible remained entire; the sand, next, formed an entire vessel considerably connected. The second crucible was also found, but in the bottom of it I found an irregular metallic mass. A small hole in the side of the interior pot had allowed nearly one-half of the original weight of iron to pass; the remainder of it, forming a very beautiful circular button, rested upon the bottom. The surface was crystallised in radii, shooting from a central point upon the upper surface of the button. The collective weight was 1858 grains: loss, 17 grains, = $\frac{1}{106}$ part the weight of the iron. When the metal was lodged in the small crucible, the sides and bottom were glazed, as in the former experiment.

III. "The same experiment was repeated with bottle glass, and a similar length and degree of heat applied. The reduction of the glass was entire. Betwixt the exterior and middle crucible, the glass appeared as an entire interposed vessel, completely surrounding the two smaller ones. Upon examining the interior crucible, I found that the contact of the bottle-glass had fused the cover of fire-clay fitted over the iron, and that the promiscuous fusion of the glass and clay formed a very fine dark-green transparent mass, beneath which lodged the metallic button. The upper surface still preserved the acuteness of the original angles upon one piece of iron; the others had resolved themselves by fusion into a very fine ingot of soft steel with partially crystallised edges. The weight of the whole was 1759 grains; less than when put in, five grains; equal to $\frac{1}{351\frac{4}{5}}$ part of the whole.—I am convinced that by prolonging the heat ten minutes, the whole iron would have entered into fusion. I would from this and other experiments infer, that the iron is of more difficult carbonation when a fused medium is made use of, than when either pure sand, pure lime, or clay, are used.

IV. "In order that no proof might be further wanting to establish the fact of the iron receiving the carbon from its so-

lution in caloric, and to obviate any objection which might probably be started against this, by supposing that the carbon might be conveyed through the porous medium of the earths, or the bottle-glass, *before* it entered into fusion, I made the following experiment:—Having made a mould in sand, I poured into it thinly fused calcareous stone; and, while the fluid was thin, introduced a small rod of red-hot iron, suspended by a very small brass wire. The dimensions of the rod were so proportioned to the mould that 5-8ths of an inch of a vitrid crust encircled the iron on all sides. The wire became fused and disengaged as the fluid was consolidating, so that every avenue to the iron was completely shut up. The mass was carefully cooled to prevent shivering, and as carefully heated, when introduced into the furnace, in a crucible inverted in one larger, and the vacancy betwixt each shut up from air by means of pounded bottle-glass. A cover was fitted on, and a violent heat urged for nearly two hours. The crucibles were taken out, free from blemish, and cemented as one compact mass. When cool, I found the metal in the interior crucible resolved into a very fine ingot, which proved afterwards to be soft steel; capable, however, of hammering and hardening to great advantage. The weight of steel obtained was 1157 grains; lost, 33 grains, = $\frac{1}{36}$ part of the whole. The fused lime, which had formerly cooled of a pure whitish porcelain colour variegated with blue streaks, was now of a spongy brown colour, exactly resembling the lava of the blast-furnace when oxygenated crude iron is produced. This alteration was unquestionably owing to the deficient weight in metal having in part been oxydated by the interior air of the crucible, or by oxygen from the furnace, and united to the earth in the state of a fused oxyde. The great difference in the loss of metal sustained in these two last experiments must be sought for in the quantity of iron taken up by the flux: in the others, the solidity and transparency of the glass were uncommonly fine; in these the flux was opaque, dark, and porous, evidently surcharged with iron.

"It will appear from these experiments, that we are still without any satisfactory or conclusive proof of the steelification of iron solely by means of the diamond.

"There

"There is one experiment which would, if succefsful, eftablifh the point. C. Clouet calculates, that, to conftitute grey crude iron, 1-6th of carbon is neceffary. Were fix parts of iron, and a diamond, equal to 1-6th of this, fubjected to the fame procefs as formerly, and the refult *grey*, or *carbonated crude iron*, inftead of fteel, the inference would be juft, and the conclufion fatisfactory.

"In the prefent ftate of my information, I doubt whether the diamond afforded even one particle of carbon to the iron. From many experiments, wherein I ufed very pure well charred wood and malleable iron, I uniformly found that the weight of the fteelified ingot nearly equalled the fum of that of the iron and carbon originally introduced: fo that, in place of having a lofs *of real iron* equal to $\frac{1}{47}$, as experienced by the Parifian chemifts, had the diamond been taken up by the iron, the ingot of fteel would have weighed within $\frac{1}{200}$ part of the aggregate weight of both, and from $\frac{1}{60}$ to $\frac{1}{70}$ part more than the iron at firft introduced.

"So far as I have gone, I think my experiments quite conclufive: the others you mention fhall be attended to in due time."

Monument to Count Rumford.

In the introduction to a German tranflation of Count Rumford's Effays, lately publifhed at Weimar, we find, among other anecdotes relative to the author, an account of a monument that was erected at Munich in the year 1795 in commemoration of his public fervices.

This monument, which is fituated in a beautiful public garden adjoining to the ramparts of the city, was erected (without the knowledge of the Count, while he was abfent from Bavaria,) by the principal inhabitants of Munich, with the permiffion and approbation of the late Elector.

The monument is a folid pile of a quadrangular form, conftructed of hewn ftone, about twenty feet in height; and it has two principal fronts, which are ornamented with fculpture and infcriptions.

Upon one of thefe fronts there is a baffo relievo of two figures

figures at whole length reprefenting the Genius of Plenty leading Bavaria by the hand, and ftrewing her path with flowers. Under this emblematical piece of fculpture, upon a broad tablet of *Bavarian* marble, there is the following infcription in the German language:—

LUSTWANDLER STEH!
DANK STÆRKET DEN GENUS.
EIN SCHŒPFERISCHER WINK CARL THEODORES,
VON MENSCHENFREUNDE RUMFORD,
MIT GEIST. GEFIHL, UND LIEB' GEFAST,
HAT DIESE EINMALS ŒDE GEGEND
IN DAS WAS DU NUN UM DICH SIHEST
VEREDELT.

Which may be thus tranflated:

Stay, Rambler!
Thankfulnefs increafes Enjoyment.
The creative glance of Charles Theodore
Rumford, the Friend of Mankind,
With Genius, Tafte, and Love infpired,
Changed this once defert place
Into what thou now beholdeft.

Upon the oppofite front of the monument there is a medallion of Count Rumford, as large as the life, reckoned a good likenefs, and under it the following infcription:—

IHM
DER DAS SCHMAEHFLICHSTE OEFFENTLICHER UEBEL,
DEN MUESSIGANG UND BETTEL TILGTE;
DER ARMUTH HILF, GEWERB UND SITTEN,
DER VATERLAENDISCHEN IUGEND
SO MANCHE BILDUNGSANSTALT GAB:
LUSTWANDLER GEH!
UND SINNE NACH IHM GLEICH ZU SEIN
AN GEIST UND THAT,
UND UNS
AN DANK.

In

In English as follows:—

To Him
Who rooted out the most disgraceful of public Evils,
Idleness and Mendicity;
Who relieved and instructed the Poor,
And founded many Institutions
For the Education of our Youth:
Go, Wanderer!
Strive to equal him
In Genius and Activity,
And us
In Gratitude!

That Count Rumford may long live to enjoy the heartfelt satisfaction that must result from the contemplation of the success of his labours in promoting the happiness of mankind, must be the sincere wish of every lover of science, and of every true friend to virtue and morality.

LUMINOUS APPEARANCE PRODUCED BY SUGAR.

In the process of breaking fine white sugar in a dark apartment, a very perceptible luminous appearance is observed, the cause of which is as much unknown to us as that produced by rubbing against each other two flints, particularly those of Iceland. The following is offered by Dr. Juch*, of Würzburg, as a kind of explanation of this phenomenon:

"While breaking refined sugar," says he, "in small pieces with a hammer and blunt knife, I suddenly perceived a very singular smell, which had a great resemblance to that of fuming nitrous acid. I looked immediately round me to see whether some vessel containing that acid might not be in the neighbourhood; but I saw none, and was certain that nothing of the kind had been in the room. I then examined the circumstance more narrowly, and found, on breaking fine sugar, that a smell similar to that of aquafortis was always perceived by every person present.

"This simultaneous disengagement of light, and of the smell of nitrous acid, I explain in the following manner:—When

* In *Scherer's Journal der Chemie*, No. 10.

sugar

ſugar is broken, a new ſurface being preſented to the air, a real acidifying proceſs inſtantly takes place: a ſmall portion of the oxygen of the atmoſphere unites itſelf with the ſugar, but not enough to form ſaccharine acid, as the quantity is not perhaps ſufficient for that purpoſe; and at the ſame moment the atmoſphere in contact with the new ſurface of the ſugar is ſo changed, in regard to the proportion of its component parts, that it affects the olfactory organs in the ſame manner as nitrous acid. This phenomenon may receive ſome farther illuſtration by repeating the experiment in a cloſe veſſel."

A SINGULAR OXYDATION OF IRON.

Dr. Juch alſo gives the following ſingular notice:—

"I had," ſays he, "a ſmall leather flaſk filled with iron filings, which I frequently uſed with my electrophorus both charged and uncharged. I had occaſion one day for a quantity of pure iron, and, having no other at hand, had recourſe to the filings in the flaſk, which I knew were pure. I emptied it on a piece of paper. I laid the paper on my hand in order to convey it to the place of its deſtination; but I had ſcarcely held it a few ſeconds when I perceived a ſtrong heat, which increaſed ſo much that I could no longer hold the paper. Some minutes after, the paper became brown, and at length took fire. It did not, however, flame. The iron filings were in a ſtate of ignition; and when the heat had decreaſed, I found the iron converted into a highly friable oxyd."

ELECTRICITY OF ICE.

The ſame author mentions, that electric ſparks may be obtained by the uſual manipulation from a cylinder of ice. Thoſe who may not be afraid of the labour, can make the experiment at the temperature of — 16½ R. = — 5·125 Fahr.

THE

PHILOSOPHICAL MAGAZINE.

DECEMBER 1799.

I. *On different Sorts of Lime used in Agriculture.* By SMITHSON TENNANT, *Esq. F.R.S.**

I WAS informed laſt ſummer, that, in the neighbourhood of Doncaſter, two kinds of lime were employed in agriculture, which were ſuppoſed to differ materially in their effects. One of theſe, which was procured near the town, it was neceſſary to uſe ſparingly, and to ſpread very evenly over the land: for it was ſaid, that a large proportion of it, inſtead of increaſing, diminiſhed the fertility of the ſoil; and that, wherever a heap of it was left in one ſpot, all vegetation was prevented for many years. Fifty or ſixty buſhels upon an acre, were conſidered to be as much as could be uſed with advantage. The other ſort of lime, which was obtained from a village near Ferry-bridge, though conſiderably dearer from the diſtant carriage, was more frequently employed, on account of its ſuperior utility. A large quantity was never found to be injurious; and the ſpots which were entirely covered with it, inſtead of being rendered barren, became remarkably fertile. The different properties aſcribed to theſe two kinds of lime were ſo very diſtinct, that it ſeemed probable they could not be imaginary; and it therefore appeared to be worth the trouble of aſcertaining them more fully, and

* From the *Phil. Tranſ. of the Royal Society for* 1799, Part II.

of attempting to difcover the nature of the ingredients from whence the difference arofe. For this purpofe I procured fome pieces of each fort of limeftone, and firft tried what would be their effect upon vegetables, in their natural ftate, by reducing them to coarfe powder, and fowing in them the feeds of different plants. In both kinds the feeds grew equally well, and nearly in the fame manner as they would in fand, or any other fubftance which affords no nourifhment to vegetables. Pieces of each fort of ftone were then burnt to lime; and, after they had been expofed for fome weeks to the air, that their caufticity might be diminifhed, fome feeds were fown in them. In the kind of lime which was found moft beneficial to land, almoft all the feeds came up, and continued to grow as long as they were fupplied with water; and the roots of the plants had many fibres, which had penetrated to the bottom of the cup in which they grew. Upon examining the compofition of this fort of lime, it proved to confift entirely of calcareous earth. By its expofure to the air for about three months, it was found to have abforbed 4-5ths of the fixed air required to faturate it. In the other kind, a few only of the feeds grew, and the plants produced from them had hardly any ftalks or roots, being formed almoft entirely of the two feed-leaves, which lay quite loofe upon the furface. This fort of lime, being fpread upon a garden foil, to the thicknefs of about the tenth of an inch, prevented nearly all the feeds which had been fown from coming up, whilft no injury was occafioned by common lime ufed in the fame manner. Upon examining the compofition of this fubftance, which was fo deftructive to the plants, it was difcovered to contain three parts of pure calcareous earth, and two of magnefia. The quantity of fixed air which it had abforbed, by being expofed for about the fame time as the pure lime juft mentioned, was only 42 hundredths of that combined with it before it was burnt.

As it feemed probable that the magnefia contained in this lime was the caufe of its peculiar properties, the following experiments were made, to determine the effects of that fubftance upon the growth of vegetables. Some feeds, chiefly of colewort, which were preferred from their growing quickly, were

were fown in uncalcined magnefia; but, though they fprouted, the leaves never rofe above the furface, and the plants were entirely without roots, nor did they appear to grow better in magnefia which had been wafhed in water containing fixed air. Calcined magnefia was, however, much more deftructive, as the feeds would not come up in it. To compare its effects on vegetables with thofe of lime, each of thefe earths was mixed, in different proportions, with fand, in fmall cups, in which feeds were then fown. The lime was obtained from marble; and, before it was put into the fand, was made to fall to powder, by being moiftened with water. In a mixture of four ounces of fand with three or four grains of calcined magnefia, it was a long time before the feeds came up, and the plants had hardly any roots or ftalks; and, with ten grains or more of magnefia, there was no appearance of vegetation. Thirty or forty grains of lime did not retard the growth of the feeds more than three or four of magnefia, and the injurious effects were not fo lafting. The lime, by abforbing fixed air, foon loft its deftructive properties; fo that, after keeping thefe mixtures four or five weeks, feeds were found to grow in that with forty grains of lime, nearly as well as in pure fand; but, in that with four grains of magnefia, they produced only the feed-leaves, as was defcribed before. It was neceffary occafionally to break in pieces the fand which had fo much lime, as it would otherwife have been too hard to admit the feeds to penetrate through it. Plants will bear a much larger proportion of magnefia in vegetable foil than in fand: with twenty grains, however, of calcined magnefia, in as much foil as was equal in bulk to four ounces of fand, the feeds produced only the feed-leaves without roots; and, with about forty grains, they were entirely prevented from coming up.

In countries where the magnefian lime is employed, it was faid, that the barrennefs of any fpot on which a heap of it had been laid, would continue for many years. To learn how far it could by time be deprived of its injurious qualities, I procured fome pieces of mortar, made of this fpecies of lime, from two houfes, one of which had been built three, and the other eight years: they were taken from the outfide

of the building, where they had been expofed to the air. After they were reduced to powder, feeds were fown in them. Only a few came up, and even thofe produced merely the feed-leaves, without any roots. As plants would grow in the limeftone from which this fpecies of lime was formed, although not in the mortar made from it, I wifhed to know what proportion of the fixed air, originally contained in the limeftone, had been abforbed by the mortar. For this purpofe a piece of it was finely powdered, to render it of an uniform quality: it was then tried how much of this powder, and of the limeftone, would faturate the fame quantity of acid: by this means I afcertained the proportions of limeftone and mortar, containing equal quantities of the magnefian lime. The fixed air being obtained from them in thofe proportions, and meafured in an inverted veffel, with quickfilver, it was found that the mortar which had been expofed three years had abforbed 43, and that of eight years only 47 hundredths of the quantity originally contained in the limeftone. I was not able to obtain any mortar which had been made earlier, though it might deferve to be known how much fixed air it was ultimately capable of abforbing. Common mortar, which had been expofed to the air for a year and three quarters, had regained 63 hundredths of its full quantity of fixed air.

As the preceding experiments were tried during the winter, in a room warmed by fire, perhaps under circumftances more favourable to vegetation, the fame quantity of magnefia would not be equally pernicious.

Magnefian limeftone may be eafily diftinguifhed from that which is purely calcareous by the flownefs of its folution in acids, which is fo confiderable, that even the fofteft kind of the former is much longer in diffolving than marble. From this property of the magnefian limeftone, there appeared to be reafon for fufpecting that the kind of marble which had been called Dolomite, from M. Dolomieu, who firft remarked its peculiarity in diffolving flowly, might alfo be fimilar in its compofition. An analyfis of this fubftance was lately given in the *Journal de Phyfique:* but this is probably erroneous; for, upon examining three fpecimens, they were

were found to confift of magnefia and calcareous earth, like the magnefian limeftone; fo that it ought no doubt to be confidered as the fame fpecies of ftone, but in a ftate of greater purity. The pieces of dolomite were from different places; one of them being found among the ruins of Rome, where it is thought to have come from Greece, as many ftatues of Grecian workmanfhip are made of it, and no quarries of a fimilar kind are known in Italy; the fecond was faid to have been thrown up by Mount Vefuvius; and the third was from Iona, one of the weftern iflands of Scotland. In many kinds of common marble, fmall particles and veins may be obferved, which are a long time in diffolving. Thefe, upon examination, I difcovered to contain a confiderable proportion of magnefia; but, as they were probably not quite free from the furrounding marble, I did not afcertain the quantity precifely.

The cryftallifed ftructure which may generally be obferved in the magnefian limeftone, feems to fhew that it has not been formed by the accidental union of the two earths, but muft have refulted from their chemical combination. The difficulty of diffolving it, may alfo arife from the attraction of the different component parts to each other. The mortar formed from this kind of lime is as foluble in acids as common marble; and the fubftances of which it confifts are eafily feparated. The magnefia may be taken from it by boiling it in muriated lime, and lime is precipitated by it from lime water; but neither of thefe effects can be produced by the ftone before it is calcined.

Magnefian limeftone is probably very abundant in various parts of England. It appears to extend for thirty or forty miles, from a little fouth-weft of Workfop in Nottinghamfhire, to near Ferry-bridge in Yorkfhire. About five or fix miles further north there is a quarry of it, near Sherburn; but whether this is a continuation from the ftratum near Ferry-bridge, I have not learnt. From fome fpecimens which were fent me, I find that the cathedral and walls of York are made of it. I have not been able to learn whether there were any fhells in the limeftone of the tract of country before-mentioned. In Mr. Marfhall's account of the agriculture of the midland counties, he fpeaks of the lime made at Breedon, near Derby, as deftructive to vegetables, when ufed

in

in large quantities. I therefore procured fome pieces of it, and they were difcovered to contain nearly the fame proportion of magnefia as that before defcribed. In this quarry the ftone is frequently cryftallifed in a rhomboidal form; and petrified fhells, not calcareous, but fimilar in compofition to the ftone itfelf, are fometimes, but very rarely, found in it. This fubftance feems to be common in Northumberland. In the third volume of the Annals of Agriculture, Dr. Fenwick, of Newcaftle, obferves, that the farmers of that country divide limes into hot and mild. The former of thefe is no doubt magnefian, as it has fimilar effects on the foil; and he remarks, that it is not fo eafily diffolved in acids as the latter. At Matlock, in Derbyfhire, the two kinds are contiguous to each other; the rocks on the fide of the river where the houfes are built being magnefian, and on the other calcareous. The magnefian rock appears alfo to be incumbent upon a calcareous ftratum; for, in defcending a cave formed in this rock, a diftinct vein of common limeftone may be obferved, which contains no magnefia. The latter ftratum is very full of fhells; but, though there are fome alfo in the magnefian rock, yet they are very rare. In the following tables, containing the analyfis of various fpecimens, fome other places are mentioned where this fubftance is found, but of which I received no further information.

After it was known that the magnefian marble and limeftone confifted of the two earths, their proportion was attempted to be difcovered, by trying how much gypfum and Epfom falt could be obtained, by means of vitriolic acid, from a certain weight of each fpecimen. When the fuperfluous vitriolic acid had been evaporated by heat, the Epfom falt was feparated from the gypfum by water. The refult of thefe trials is expreffed in the following table:—

	Dry Gypfum.	Dry Epfom Salt.
5 grains of limeftone from Breedon gave	3.9	3.15
——————— Matlock -	3.95	2.9
——————— Workfop -	3.8	3.0
——————— York -	3.8	3.1
3 grains of calcareous fpar, and 1 grain of calcined magnefia, gave -	3.9	2.7

As

As the preceding method of estimating the quantities of magnesia and calcareous earth is liable to considerable error, I afterwards examined them in the following manner, which seems capable of great exactness:—Twenty-five grains of each substance were dissolved, by marine acid, in a cup of platina, and, after the solution was evaporated to dryness, it was made red-hot for a few minutes. The mass remaining in the cup, which consisted of muriated lime, and of the magnesia freed from the acid, was washed out with water, and poured into a phial. There was then added to it a known quantity of diluted marine acid, somewhat more than was sufficient to redissolve the magnesia, and, after the solution, a certain weight of calcareous spar, part of which would be dissolved by the superfluous acid. By the quantity of spar remaining undissolved, it was learnt how much acid was required to dissolve the magnesia. The iron and argillaceous earth contained in some specimens, were precipitated by the spar, and therefore could not occasion any error. The calcareous spar, however, dissolved more slowly where there was argillaceous earth, as it became coated with it; but this incrustation was occasionally removed, and, in all the experiments, the spar was left in the solution till it suffered no further diminution. For this purpose it was necessary to keep them slightly warm for some days, during which time the phials were generally closed, to prevent any escape of the acid.

The first experiment in the following table was made upon known quantities of magnesia and calcareous earth, to try the accuracy of the process. For this purpose, also, the second was repeated upon a piece of limestone, previously powdered, to render every part of it of the same quality. The first column shews the quantity of calcareous spar which might have been dissolved by the acid required to take up the magnesia. The second shews the corresponding quantities of magnesia in 25 grains of each substance. The third expresses the quantity of lime. This was inferred by subtracting the weight of the magnesia, and of the iron and clay, from 13.2 grains, the weight of the whole quantity of earth in 25 grains of limestone. This is probably not very incorrect,

incorrect, as, in two ſpecimens which differed moſt in the proportion of magneſia and lime, the weight of the two earths was nearly the ſame.

A piece of dolomite, from Rome, was wrapped in a thin leaf of platina, that no part of it might be loſt, and, being then expoſed to a ſtrong heat, left of earth - 52.9 *per cent.*

Dolomite from Mount Veſuvius -	52.8
Breedon limeſtone - -	52.4
Calcareous ſpar left of lime - -	55.8

In three of the experiments, alſo, the calcareous earth was precipitated by mineral alkali; and the quantity of it being tried by that of the marine acid required to diſſolve it, it correſponded very nearly with that put down.

A quantity of marine acid which would diſſolve 15 grains of calcareous ſpar, would alſo diſſolve 5.5 of calcined magneſia, and 2.5 grains of ſpar; ſo that 12.5 grains of ſpar, required the ſame quantity of acid as 5.5 grains of magneſia.

The magneſia uſed was very pure, and made red-hot immediately before it was weighed.

Subſtances examined.	Quantity of ſpar which the acid, required to take up the magneſia, would have diſſolved.	Quantity of magneſia.	Quantity of lime.	Iron and clay.	
Mixture of 5.5 grains of magneſia and 14 grains of calcareous ſpar - - -	12.5	5.5	7.8	0	
25 grains of Breedon limeſtone, previouſly powdered - -	11.53	5.071	7.929	.2	
25 grains from part of the ſame powder - - -	11.56	5.082	7.913	.2	
25 grains of dolomite from Rome - - -	12.2	5.37	7.73	.1	
——— dolomite from Iona - - - -	10.1	4.4	7.8	1.0	Infuſible ſubſtance.
——— Veſuvian dolomite - - - -	10.38	4.565	8.575	.06	
A ſecond experiment, from part of the ſame Veſuvian dolomite - - -	10.03	4.411	8.849	.06	
25 grains of magneſian limeſtone from Wanſworth, near Doncaſter - - -	12.75	5.61	7.34	.25	
——— Thorpe arch -	10.95	4.84	7.8	.6	
——— Matlock - -	12.5	5.5	7.388	.31	
——— York-minſter -	11.	4.84	8.26	.1	
——— Workſop - -	11.6	5.104	7.496	.6	
——— Sherburn - -	11.5	5.08	7.56	.56	
——— Weſtminſter-hall	10.1	4.44	8.37	.4	

II. *Agenda,*

II. *Agenda, or a Collection of Obſervations and Reſearches, the Reſults of which may ſerve as the Foundation for a Theory of the Earth. By* M. DE SAUSSURE.

[Concluded from Page 140.]

CHAP. XXIII.

Inſtruments neceſſary for the Geological Traveller.

1. THE moſt neceſſary inſtrument is the miner's hammer. It will be requiſite to have two, of different ſizes: one ſmall, to break ſmall fragments of rolled pebbles, by holding them in the left hand while you ſtrike with the right. Its weight, including the handle, ought to be about ten ounces. The other muſt be larger, to detach fragments of rock, and to break large pebbles: its weight ought to be quadruple that of the ſmall one. When I travel on horſeback, I have theſe two hammers ſuſpended from the bow of my ſaddle.

1. A. Two ſtone-cutter's chiſels: one ſmall, of from a line to a line and a half, to detach ſmall cryſtals, or other objects of ſmall bulk; the other, ſeven or eight lines.

2. To try the hardneſs of foſſils, a piece of ſteel to ſtrike fire will be neceſſary; alſo a triangular file, pretty fine, and a ſtrong bodkin of tempered ſteel.

3. Nitrous acid, with M. De Morveau's boxes of reagents.

3. A. An artificial magnet, in a caſe, with a ſteel pivot on which it can be placed, to try the magnetiſm of foſſils.

4. A magnifying glaſs of three inches focus, in order to enable the obſerver to form a general idea of any foſſil: another, of an inch focus, to examine its ſeparated parts; and one of five or ſix lines for cloſer examination. Theſe three magnifiers muſt be always in the traveller's pocket, or ready at hand: but, beſides theſe, he muſt have, for his cloſet at home, a microſcope furniſhed with a micrometer.

5. Teleſcopes, to obſerve inacceſſible mines and diſtant mountains.

6. A pocket portfolio, with prepared paper for writing on,

with a pencil of tin folder, which it is not neceſſary always to cut, and the writing of which is not ſo eaſily effaced as that of plumbago. In this portfolio the traveller muſt write out, on the ſpot, the ſketch of his journal, and inſert ſuch obſervations as occur to him; but he muſt take the trouble to tranſcribe theſe notes at more length, preſerving the primitive notes, which will always retain a character of truth, and for that reaſon people are fond of recurring to them.

7. Some quires of brown paper, a few ſheets of which may be carried in the pocket for wrapping up on the ſpot ſpecimens of the ſtones you collect, the characters of which ought to be marked on the cover. You may afterwards pack them with hay into a bag deſtined for that purpoſe, until you have a ſufficient quantity to form a box, which you may ſend home by the public carriages wherever you find an oportunity; but, as it is fatiguing for the traveller to load his pockets during the time of his excurſions, and as the guides often loſe them on purpoſe in order to get rid of them, I have behind my ſaddle two leathern bags, into which I put them till I come to ſome halting-place, where I have time to pack them with hay into a bag. M. Beſſon recommends to thoſe who undertake ſea voyages to write with China ink the characters which ought to accompany minerals in long paſſages, becauſe common ink may be effaced by accidents.

8. A blow-pipe, with its apparatus. As I make much uſe of this inſtrument, which at length fatigues me, though I can blow with my cheeks without exerciſing my breaſt, I cauſed to be conſtructed a pair of portable double bellows, the ſides of which contain each ſixty-two ſquare inches. Theſe bellows can be ſuſpended from the edge of a table; and I put them in motion by preſſing together, between my knees, the two handles, which afterwards ſeparate by the action of the ſpring. This apparatus may be eaſily carried, and is very convenient.

9. A graduated ſemicircle traced out on a copper-plate of a form exactly rectangular, with a plummet ſuſpended from the centre of the ſemicircle. This ſemicircle is the moſt convenient inſtrument for meaſuring the inclination of ſtrata, of veins and declivities; and it may always be carried in a pocket of the portfolio.

10. A

10. A compafs, furnifhed with a crofs ftaff, to find the direction of mountains, chains, vallies, and ftrata.

11. Portable barometers with two mercurial thermometers; one affixed to a barometer to eftimate the temperature of the mercury in the latter, and the other with a bare bulb for meafuring the temperature of the air. Thofe who ftudy meteorology, as well as geology, ought to be furnifhed alfo with an hygrometer and an electrometer.

12. For afcertaining the temperature of the fea, at great depths, it will be neceffary to have a thermometer conftructed like that defcribed in my Travels through the Alps *: for lakes, an apparatus like that pointed out in the note of Section 1399, will be fufficient.

13. Thofe who underftand a little geometry, ought to provide themfelves with a fextant, having an artificial horizon, and alfo a chain, in order that they may be able to meafure a bafe, and thus take the altitude of an inacceffible peak, the breadth of a river, &c. &c. With this fextant they may alfo find the latitudes. In regard to the longitudes, they require, befides inftruments, an expertnefs in this kind of obfervation, which cannot be attained but by mariners or profeffed aftronomers.

14. It will be neceffary alfo to have within reach tools for repairing an inftrument in cafe it fhould happen to be deranged; fuch as pincers, files, turnfcrews, compaffes, gimblets, wire, needles, thread, and packthread.

15. Laftly, fome good map, pafted on canvas, of the country you propofe to examine; and this map ought frequently to be compared with your itinerary, and the bearings given by your compafs.

16. In regard to the care required for the traveller's perfon, he muft have a light drefs made of cloth, without lining, of a white colour, as well as his hat, that he may be lefs expofed to the heat of the fun's rays; with jackets, fome cool for the warm regions and the vallies, and the other warm for the cool regions and eminences; a good great coat; green fpectacles, and a black crape, to fecure the eyes and face from the fnow. Laftly, if he is to pafs the night in the open air, a

* Section 1392, Plate 1, fig. 3.

tent or *cannoniere*, a bear's ſkin to ſleep upon, and woollen blankets.

17. A ſolid light walking-pole: mine for the high Alps is a young fir-plant, extremely dry, ſeven feet in length and 18 lines in diameter at the lower end, which is ſhod with iron tapering to a ſtrong point. Theſe dimenſions may appear large, but nothing can be too ſtrong for the ſteep rocks, glaciers, and ſnow, when you are obliged to take your point of ſupport at a diſtance from you, and to reſt the whole weight of your body on your pole, by holding it in a ſituation very much inclined, and even horizontal, as may be ſeen in the Vignette to the Firſt Volume of my Travels through the Alps. —For mountains which are not ſo ſteep, the traveller may be ſatisfied with poles of leſs ſtrength and ſize; but it will ſtill be neceſſary that they ſhould be four or five feet in height, and ſufficiently ſtrong that a perſon might be able to ſupport himſelf with his two hands by holding them in a horizontal ſituation, according to the attitude of the ſmall figure which is on the left ſide at top of the before-mentioned vignette; for, in traverſing or deſcending a rapid declivity, or in walking on the margin or edge of a precipice, the traveller muſt always ſupport himſelf by his two hands, holding the pole towards the mountain, and not towards the precipice, as thoſe do who have not learned the art of travelling through mountains.

18. To prevent ſlipping on the hard ſnow-ice, and graſs-plats, which are ſtill more dangerous, I would recommend iron cramps, ſuch as thoſe which I have cauſed to be engraved in the third plate of the firſt volume, and which I have long uſed with ſucceſs. In my laſt excurſions, however, I preferred ſhoes, the thick ſoles of which were armed with ſtrong tacks, at the diſtance of eight or nine lines from each other. The heads of theſe tacks are of ſteel, and ſhaped like a ſquare pyramid: I have ſome ſmall ones, the points of which are only two lines and a half in height, and of about the ſame breadth, for the ſnow-rocks and graſs-plats; and others, of double theſe dimenſions, for the hard ſnow.

19. In the laſt place, with regard to proviſions, when the traveller muſt reſide for a conſiderable time in the deſarts, at a diſtance from habitations, and even huts, he may carry with

with him fome falt or pickled meat; but M. Parmentier's faloop of potatoes, with cakes of portable foup, and bread, will form the moft nourifhing food, and what may be contained in the leaft room. A fmall iron chaffing-difh, a fmall bag filled with charcoal, and a pan of tinned copper or iron, form my kitchen apparatus for the mountains: wooden plates and fpoons may be found in the remoteft huts. It will be proper, however, to carry always in the pocket a cup of gum elaftic, in order that the traveller may at all times eafily quench his thirft, a want to which he will be frequently expofed in his excurfions.

From what has been faid, it may be readily feen, that the ftudy of geology will not fuit the indolent or fenfual; for the life of the geologue muft be divided between fatiguing and perilous journies, in which he is deprived of almoft all the conveniences of life, and the varied and deep refearches of the clofet. But what is ftill more rare, and perhaps more neceffary than the zeal requifite to furmount thefe obftacles, is, a mind free from prejudice, filled with an ardent defire for the truth alone, rather than with a defire for raifing or deftroying fyftems; capable of defcending to minute details indifpenfibly neceffary for the correctnefs and certainty of obfervations, and of rifing to grand views and general conceptions. Thofe fond of fuch ftudies, ought not, however, to be difcouraged by thefe difficulties; there is no traveller who may not make fome good obfervation, and bring with him at leaft one ftone worthy of being employed in the conftruction of this grand edifice. It is indeed poffible to be ufeful without attaining to perfection; for I have no doubt that if the mineralogical travels, even the moft efteemed, and much more thofe of the author, be compared with thefe Agenda, there will be found in them many deficiencies, and many obfervations, either imperfect, or even totally forgotten: but I have mentioned the reafon in the Introduction. Befides, feveral of thefe ideas did not occur till I had finifhed my travels; and for that reafon I laboured with more zeal on thefe Agenda, in the hope of rendering young perfons, on their entering this career, capable of performing what coft me thirty-fix years of travelling and ftudy.

III. *On*

III. *On the Nature and Construction of the Sun and Fixed Stars.* *By* William Herschel, *LL.D. F.R.S.*

[Concluded from Page 123.]

IT will now be eaſy to bring the reſult of theſe obſervations into a very narrow compaſs. That the ſun has a very extenſive atmoſphere cannot be doubted; and that this atmoſphere conſiſts of various elaſtic fluids, that are more or leſs lucid and tranſparent, and of which the lucid one is that which furniſhes us with light, ſeems alſo to be fully eſtabliſhed by all the phænomena of its ſpots, of the faculæ, and of the lucid ſurface itſelf. There is no kind of variety in theſe appearances but what may be accounted for with the greateſt facility, from the continual agitation which, we may eaſily conceive, muſt take place in the regions of ſuch extenſive elaſtic fluids.

It will be neceſſary, however, to be a little more particular as to the manner in which I ſuppoſe the lucid fluid of the ſun to be generated in its atmoſphere. An analogy that may be drawn from the generation of clouds in our own atmoſphere, ſeems to be a very proper one, and full of inſtruction. Our clouds are probably decompoſitions of ſome of the elaſtic fluids of the atmoſphere itſelf, when ſuch natural cauſes, as in this grand chemical laboratory are generally at work, act upon them: we may therefore admit, that in the very extenſive atmoſphere of the ſun, from cauſes of the ſame nature, ſimilar phænomena will take place; but with this difference, that the continual and very extenſive decompoſitions of the elaſtic fluids of the ſun are of a phoſphoric nature, and attended with lucid appearances, by giving out light.

If it ſhould be objected, that ſuch violent and unremitting decompoſitions would exhauſt the ſun, we may recur again to our analogy, which will furniſh us with the following reflections. The extent of our own atmoſphere, we ſee, is ſtill preſerved, notwithſtanding the copious decompoſitions of its fluids in clouds and falling rain; in flaſhes of lightning, in meteors, and other luminous phænomena; becauſe there are freſh ſupplies of elaſtic vapours continually aſcending to make

good the waste occasioned by those decompositions. But it may be urged, that the case with the decomposition of the elastic fluids in the solar atmosphere would be very different, since light is emitted, and does not return to the sun, as clouds do to the earth when they descend in showers of rain. To which I answer, that, in the decomposition of phosphoric fluids, every other ingredient but light may also return to the body of the sun. And that the emission of light must waste the sun, is not a difficulty that can be opposed to our hypothesis: for, as it is an evident fact that the sun does emit light, the same objection, if it could be one, would equally militate against every other assignable way to account for the phænomenon.

There are, moreover, considerations that may lessen the pressure of this alledged difficulty. We know the exceeding subtilty of light to be such, that in ages of time its emanation from the sun cannot very sensibly lessen the size of this great body. To this may be added, that, very possibly, there may also be ways of restoration to compensate for what is lost by the emission of light, though the manner in which this can be brought about should not appear to us. Many of the operations of Nature are carried on in her great laboratory which we cannot comprehend, but now and then we see some of the tools with which she is at work. We need not wonder that their construction should be so singular as to induce us to confess our ignorance of the method of employing them, but we may rest assured that they are not a mere *lusus naturæ*. I allude to the great number of small telescopic comets that have been observed, and to the far greater number still that are probably much too small for being noticed by our most diligent searchers after them. Those six, for instance, which my sister has discovered, I can from examination affirm, had not the least appearance of any solid nucleus, and seemed to be mere collections of vapours condensed about a centre. Five more, that I have also observed, were nearly of the same nature. This throws a mystery over their destination, which seems to place them in the allegorical view of tools, probably designed for some salutary purposes to be wrought by them; and, whether the restoration of what is

lost

loſt to the ſun by the emiſſion of light, the poſſibility of which we have been mentioning above, may not be one of theſe purpoſes, I ſhall not preſume to determine. The motion of the comet, diſcovered by Mr. Meſſier in June 1770, plainly indicated how much its orbit was liable to be changed by the perturbations of the planets; from which, and the little agreement that can be found between the elements of the orbits of all the comets that have been obſerved, it appears clearly that they may be directed to carry their ſalutary influence to any part of the heavens.

My hypotheſis, however, as before obſerved, does not lay me under any obligation to explain how the ſun can ſuſtain the waſte of light, nor to ſhew that it will ſuſtain it for ever; and I ſhould alſo remark that, as in the analogy of generating clouds, I merely allude to their production as owing to a decompoſition of ſome of the elaſtic fluids of our atmoſphere, that analogy, which firmly reſts upon the fact, will not be leſs to my purpoſe, to whatever cauſe theſe clouds may owe their origin. It is the ſame with the lucid clouds, if I may ſo call them, of the ſun. They plainly exiſt, becauſe we ſee them; the manner of their being generated may remain an hypotheſis—and mine, till a better can be propoſed, may ſtand good; but, whether it does or not, the conſequences I am going to draw from what has been ſaid will not be affected by it.

Before I proceed I ſhall only point out, that, according to the above theory, a dark ſpot in the ſun is a place in its atmoſphere which happens to be free from luminous decompoſitions; and that faculæ are, on the contrary, more copious mixtures of ſuch fluids as decompoſe each other. The penumbra, which attends the ſpots, being generally depreſſed, more or leſs, to about half way between the ſolid body of the ſun and the upper part of thoſe regions in which luminous decompoſitions take place, muſt of courſe be fainter than other parts. No ſpot favourable for taking meaſures having lately been on the ſun, I can only judge, from former appearances, that the regions in which the luminous ſolar clouds are formed, adding thereto the elevation of the faculæ, cannot be leſs than 1843, nor much more than 2765 miles

miles in depth. It is true that in our atmoſphere the extent of the clouds is limited to a very narrow compaſs; but we ought rather to compare the ſolar ones to the luminous decompoſitions which take place in our *aurora borealis*, or luminous arches which extend much farther than the cloudy regions. The denſity of the luminous ſolar clouds, though very great, may not be exceedingly more ſo than that of our *aurora borealis*. For, if we conſider what would be the brilliancy of a ſpace two or three thouſand miles deep, filled with ſuch corruſcations as we ſee now and then in our atmoſphere, their apparent intenſity, when viewed at the diſtance of the ſun, might not be much inferior to that of the lucid ſolar fluid.

From the luminous atmoſphere of the ſun I proceed to its opaque body, which, by calculation from the power it exerts upon the planets, we know to be of great ſolidity; and from the phænomena of the dark ſpots, many of which, probably on account of their high ſituations, have been repeatedly ſeen, and otherwiſe denote inequalities in their level, we ſurmiſe that its ſurface is diverſified with mountains and vallies.

What has been ſaid enables us to come to ſome very important concluſions, by remarking, that this way of conſidering the ſun and its atmoſphere removes the great diſſimilarity we have hitherto been uſed to find between its condition and that of the reſt of the great bodies of the ſolar ſyſtem.

The ſun, viewed in this light, appears to be nothing elſe than a very eminent, large, and lucid planet, evidently the firſt, or, in ſtrictneſs of ſpeaking, the only primary one of our ſyſtem, all others being truly ſecondary to it. Its ſimilarity to the other globes of the ſolar ſyſtem with regard to its ſolidity, its atmoſphere, and its diverſified ſurface, the rotation upon its axis, and the fall of heavy bodies, leads us on to ſuppoſe that it is moſt probably alſo inhabited, like the reſt of the planets, by beings whoſe organs are adapted to the peculiar circumſtances of that vaſt globe.

Whatever fanciful poets might ſay in making the ſun the abode of bleſſed ſpirits, or angry moraliſts deviſe in pointing it out as a fit place for the puniſhment of the wicked, it does

not appear that they had any other foundation for their aſſertions than mere opinion and vague ſurmiſe; but now I think myſelf authoriſed, *upon aſtronomical principles*, to propoſe the ſun as an inhabitable world; and am perſuaded that the foregoing obſervations, with the concluſions I have drawn from them, are fully ſufficient to anſwer every objection that may be made againſt it.

It may, however, not be amiſs to remove a certain difficulty, which ariſes from the effect of the ſun's rays upon our globe. The heat which is here, at the diſtance of 95 millions of miles, produced by theſe rays, is ſo conſiderable, that it may be objected, that the ſurface of the globe of the ſun itſelf muſt be ſcorched up beyond all conception.

This may be very ſubſtantially anſwered by many proofs drawn from natural philoſophy, which ſhew that heat is produced by the ſun's rays only when they act upon a calorific medium; they are the cauſe of the production of heat, by uniting with the matter of fire which is contained in the ſubſtances that are heated; as the colliſion of flint and ſteel will inflame a magazine of gunpowder, by putting all the latent fire it contains into action. But an inſtance or two of the manner in which the ſolar rays produce their effect, will bring this home to our moſt common experience.

On the tops of mountains of a ſufficient height, at an altitude where clouds can very ſeldom reach to ſhelter them from the direct rays of the ſun, we always find regions of ice and ſnow. Now, if the ſolar rays themſelves conveyed all the heat we find on this globe, it ought to be hotteſt where their courſe is leaſt interrupted. Again, our aëronauts all confirm the coldneſs of the upper regions of the atmoſphere; and ſince, therefore, even on our earth, the heat of any ſituation depends upon the aptneſs of the medium to yield to the impreſſion of the ſolar rays, we have only to admit, that, on the ſun itſelf, the elaſtic fluids compoſing its atmoſphere, and the matter on its ſurface, are of ſuch a nature as not to be capable of any exceſſive affection from its own rays: and, indeed, this ſeems to be proved by the copious emiſſion of them; for if the elaſtic fluids of the atmoſphere, or the matter contained on the ſurface of the ſun, were of ſuch a nature

as to admit of an eaſy, chemical combination with its rays, their emiſſion would be much impeded.

Another well known fact is, that the ſolar focus of the largeſt lens, thrown into the air, will occaſion no ſenſible heat in the place where it has been kept for a conſiderable time, although its power of exciting combuſtion, when proper bodies are expoſed, ſhould be ſufficient to fuſe the moſt refractory ſubſtances *.

It will not be neceſſary to mention other objections, as I can think of none that may be made but what a proper conſideration of the foregoing obſervations will eaſily remove; ſuch as may be urged from the diſſimilarity between the luminous atmoſphere of the ſun and that of our globe will be touched upon hereafter, when I conſider the objections that may be aſſigned againſt the moon's being an inhabitable ſatellite.

I ſhall now endeavour, by analogical reaſonings, to ſupport the ideas I have ſuggeſted concerning the conſtruction and purpoſes of the ſun; in order to which it will be neceſſary to begin with ſuch arguments as the nature of the caſe will admit, to ſhew that our moon is probably inhabited. This ſatellite is of all the heavenly bodies the neareſt, and therefore moſt within the reach of our teleſcopes: accordingly we find, by repeated inſpection, that we can with perfect confidence give the following account of it:—

It is a ſecondary planet, of a conſiderable ſize; the ſurface of which is diverſified, like that of the earth, by mountains and vallies. Its ſituation with reſpect to the ſun is much like that of the earth, and, by a rotation on its axis, it enjoys an agreeable variety of ſeaſons, and of day and night. To the moon our globe will appear to be a very capital ſatellite, undergoing the ſame regular changes of illuminations as the moon does to the earth. The ſun, the planets, and the ſtarry conſtellations of the heavens, will riſe and ſet there as they do here; and heavy bodies will fall on the moon as they do

* The ſubject of light and heat has been very ably diſcuſſed by Mr. De Luc, in his excellent work, *Idées ſur la Météorologie*, Tome I. part 2. chap. 2. ſection 2. *De la Nature du Feu*; and Tome II. part 3. chap. 6. ſection 1. *Des Rapports de la Lumière avec la Chaleure dans l'Atmoſphère.*

on

on the earth. There seems only to be wanting, in order to complete the analogy, that it should be inhabited like the earth.

To this it may be objected, that we perceive no large seas in the moon: that its atmosphere (the existence of which has even been doubted by many) is extremely rare, and unfit for the purposes of animal life: that its climates, its seasons, and the length of its days, totally differ from ours: that without dense clouds (which the moon has not), there can be no rain; perhaps no rivers, no lakes. In short, that, notwithstanding the similarity which has been pointed out, there seems to be a decided difference in the two planets we have compared.

My answer to this will be, that that very difference which is now objected will rather strengthen the force of my argument than lessen its value: we find, even upon our globe, that there is the most striking difference in the situation of the creatures that live upon it. While man walks upon the ground, the birds fly in the air, and fishes swim in water; we can certainly not object to the conveniences afforded by the moon, if those that are to inhabit its regions are fitted to their conditions as well as we on this globe are to ours. An absolute, or total sameness, seems rather to denote imperfections, such as Nature never exposes to our view; and, on this account, I believe the analogies that have been mentioned fully sufficient to establish the high probability of the moon's being inhabited like the earth.

To proceed, we will now suppose an inhabitant of the moon, who has not properly considered such analogical reasonings as might induce him to surmise that our earth is inhabited, were to give it as his opinion that the use of that great body, which he sees in his neighbourhood, is to carry about his little globe, that it may be properly exposed to the light of the sun, so as to enjoy an agreeable and useful variety of illumination, as well as to give it light by reflection from the sun when direct day-light cannot be had. Suppose also that the inhabitants of the satellites of Jupiter, Saturn, and the Georgian planet, were to look upon the primary ones, to which they belong, as mere attractive centres, to keep together their orbits, to direct their revolution round the sun, and

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to ſupply them with reflected light in the abſence of direct illumination. Ought we not to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the earth, and thoſe other planets that have ſatellites about them, perform all the offices that have been named for the inhabitants of theſe little globes; but to us, who live upon one of theſe planets, their reaſonings cannot but appear very defective, when we ſee what a magnificent dwelling-place the earth affords to numberleſs intelligent beings.

Theſe conſiderations ought to make the inhabitants of the planets wiſer than we have ſuppoſed thoſe of their ſatellites to be. We ſurely ought not, like them, to ſay "the ſun "(that immenſe globe, whoſe body would much more than "fill the whole orbit of the moon) is merely an attractive "centre to us." From experience we can affirm, that the performance of the moſt ſalutary offices to inferior planets is not inconſiſtent with the dignity of ſuperior purpoſes; and, in conſequence of ſuch analogical reaſonings, aſſiſted by teleſcopic views, which plainly favour the ſame opinion, we need not heſitate to admit that the ſun is richly ſtored with inhabitants.

This way of conſidering the ſun is of the utmoſt importance in its conſequences. That ſtars are ſuns can hardly admit of a doubt. Their immenſe diſtance would perfectly exclude them from our view, if the light they ſend us were not of the ſolar kind. Beſides, the analogy may be traced much farther. The ſun turns on its axis: ſo does the ſtar Algol: ſo do the ſtars called β Lyræ, δ Cephei, η Antinoi, o Ceti, and many more; moſt probably all. From what other cauſe can we ſo probably account for their periodical changes? Again, our ſun has ſpots on its ſurface: ſo has the ſtar Algol, and ſo have the ſtars already named, and probably every ſtar in the heavens. On our ſun theſe ſpots are changeable: ſo they are on the ſtar o Ceti, as evidently appears from the irregularity of its changeable luſtre, which is often broken in upon by accidental changes while the general period continues unaltered. The ſame little deviations have been obſerved in other periodical ſtars, and ought to be

aſcribed

ascribed to the same cause. But if stars are suns, and suns are inhabitable, we see at once what an extensive field for animation opens itself to our view.

It is true that analogy may induce us to conclude, that, since stars appear to be suns, and suns, according to the common opinion, are bodies that serve to enlighten, warm, and sustain a system of planets, we may have an idea of numberless globes that serve for the habitation of living creatures. But if these suns themselves are primary planets, we may see some thousands of them with our own eyes, and millions by the help of telescopes, when at the same time the same analogical reasoning still remains in full force with regard to the planets which these suns may support.

In this place I may, however, take notice, that, from other considerations, the idea of suns or stars being *merely* the supporters of systems of planets, is not absolutely to be admitted as a general one. Among the great number of very compressed clusters of stars I have given in my catalogues, there are some which open a different view of the heavens to us. The stars in them are so very close together, that, notwithstanding the great distance at which we may suppose the cluster itself to be, it will hardly be possible to assign any sufficient mutual distance to the stars composing the cluster, to leave room for crowding in those planets, for whose support these stars have been, or might be, supposed to exist. It should seem, therefore, highly probable that they exist for themselves; and are, in fact, only very capital, *lucid*, primary planets, connected together in one great system of mutual support.

As in this argument I do not proceed upon conjectures, but have actual observations in view, I shall mention an instance in the clusters No. 26, 28, and 35, VI. class, of my catalogue of nebulæ, and clusters of stars. (See Phil. Transf. Vol. LXXIX. Part II. p. 251.) The stars in them are so crowded that I cannot conjecture them to be at a greater apparent distance from each other than five seconds, even after a proper allowance for such stars, as, on a supposition of a globular form of the cluster, will interfere with one another, has been made. Now, if we would leave as much room be-

tween

tween each of theſe ſtars as there is between the Sun and Sirius, we muſt place theſe cluſters 42104 times as far from us as that ſtar is from the ſun. But, in order to bring down the luſtre of Sirius to that of an equal ſtar placed at ſuch a diſtance, I ought to reduce the aperture of my 20-feet teleſcope to leſs than the two-and-twenty hundredth part of an inch; when certainly I could no longer expect to ſee any ſtar at all.

The ſame remark may be made with regard to the number of very cloſe double ſtars, whoſe apparent diameters being alike, and not very ſmall, do not indicate any very great mutual diſtance: from which, however, muſt be deducted all thoſe where the different diſtances may be compenſated by the real difference in their reſpective magnitudes.

To what has been ſaid may be added, that, in ſome parts of the milky way, where yet the ſtars are not very ſmall, they are ſo crowded, that in the year 1792, Aug. 22, I found by the gages that, in 41 minutes of time, no leſs than 258 thouſand of them had paſſed through the field of view of my teleſcope *.

It ſeems, therefore, upon the whole, not improbable that, in many caſes, ſtars are united in ſuch cloſe ſyſtems as not to leave much room for the orbits of planets or comets; and that conſequently, upon this account alſo, many ſtars, unleſs

* The ſtar-gages ran thus:

From 19^h	$35'$	to 19^h	$51'$	600 ſtars in the field
19	51	— 19	57	440
19	57	— 20	12	360
20	12	— 20	16	260

The breadth of the ſweep was $2° 35'$, the diameter of the field $15'$, and the mean polar diſtance $73° 54'$. Then let

F, be the diameter of the field of view,

S, the number of ſtars in each field,

B, the breadth of the ſweep, plus F,

T, the length of the ſweep, expreſſed in minutes of ſpace,

ϕ the ſine of the mean polar diſtance,

C, the conſtant fraction ,7854,

and the ſtars in theſe four ſucceſſive ſhort ſweeps will be found by the expreſſion $\frac{BTS\phi}{F^2C}$ equal to 133095. 36601. 74866. 14419. or in all 258981.

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we would make them mere uſeleſs brilliant points, may themſelves be lucid planets, perhaps unattended by ſatellites.

POSTSCRIPT.

The following obſervations, which were made with an improved apparatus, and under the moſt favourable circumſtances, ſhould be added to thoſe which have been given. They are deciſive with regard to one of the conditions of the lucid matter of the ſun.

Nov. 26, 1794. Eight ſpots in the ſun, and ſeveral ſubdiviſions of them, are all equally depreſſed.

The ſun is mottled every where.

The mottled appearance of the ſun is owing to an inequality in the level of the ſurface.

The ſun is equally mottled at its poles and at its equator; but the mottled appearances may be ſeen better about the middle of the diſc than towards the circumference, on account of the ſun's ſpherical form.

The unevenneſs ariſing from the elevation and depreſſion of the mottled appearance on the ſurface of the ſun, ſeems in many places to amount to as much, or to nearly as much, as the depreſſion of the penumbræ of the ſpots below the upper part of the ſhining ſubſtance, without including faculæ, which are protuberant.

The lucid ſubſtance of the ſun is neither a liquid nor an elaſtic fluid; as is evident, from its not inſtantly filling up the cavities of the ſpots, and of the unevenneſs of the mottled parts. It exiſts, therefore, in the manner of lucid clouds ſwimming in the tranſparent atmoſphere of the ſun; or rather, of luminous decompoſitions taking place within that atmoſphere.

IV. *Extract*

IV. *Extract of a Memoir, and Experiments on the Nutrition of Plants. By M.* Rafn, *Assessor in the Office of Commerce at Copenhagen**.

HASSENFRATZ confiders carbon as the fubftance which nourifhes vegetables †. Ingenhous, in his work on the nutrition of plants, publifhed in 1797, endeavours to prove, that if carbon has any influence in this refpect, it can be only in the ftate of carbonic acid, as that acid is abforbed and decompofed ‡ by vegetables; while the ligneous carbon, furnifhed by Nature, produces no effect on the expanfion of plants. Mr. A. Young has endeavoured to demonftrate the fame thing by experiments. M. Rafn, defirous of difcovering the truth amidft thefe contradictory opinions, made, for three years, a feries of experiments, from which he concludes, by the expanfion, fize, and colour of the plants employed, that carbon, either vegetable or animal, has a decided influence in the nourifhment of vegetables. What is new, and particularly worthy of remark in thefe refearches, is, that, according to M. Rafn, the carbonic acid produces exactly the fame effect as charcoal of wood. The following are the experiments which conducted the author to this refult:—

Having half filled a large box with brick-kiln rubbifh, or pounded tiles, which he covered with a layer of vegetable earth, he placed over the latter a ftratum of carbonate of lime (pounded limeftone) and alum, and then two or three of vegetable mould, in which he fowed barley. He prefumed that the fulphuric acid of the alum, abandoning the argil to join the lime, with which this acid has greater affi-

* Tranflated from the Danifh, with notes by C. Vauquelin and Brogniart.

† This opinion of C. Haffenfratz appears to be very probable; but, as he fays, the carbon muft be held in folution by hydrogen gas, by water, by that faponaceous extract which feparates from vegetables when in a ftate of putrefaction, or by any other liquid.

‡ This decompofition is poffible, but it has not yet been proved by any direct experiment.

 nity,

nity, the carbonic acid gas would be disengaged, which would furnish the means of knowing its influence on the vegetation. Another box was filled merely with mould, a third merely with charcoal, and a fourth with animal carbon. These were to be employed in comparative experiments, and barley was sown in them all.

Though the plants which germinated in the first box were sown in a stratum of mould about two or three inches in thickness, they had no resemblance, either in strength or colour, to those sown in the second box filled with mould alone; but they had, on the other hand, such a perfect resemblance to those of the third box filled with charcoal, that it would have been difficult to distinguish the difference. This resemblance continued several weeks, after which they seemed to have not quite the same vigour as those which grew in the charcoal, for which it is not difficult to assign a reason. The author convinced himself that a decomposition had really taken place, because, on examining the first box in autumn, he found that sulphat of lime had been formed. These experiments seem proper to conduct to a knowledge of the manner in which plants attract the carbonic principle, which all the researches of the author demonstrate to be necessary for vigorous vegetation. He proposes to repeat them on a larger scale, and to vary them as much as possible *. He repeated, several times, those of M. Humboldt on germination, accelerated by the oxygenated muriatic acid, and always with success, though with this difference, that this acid did not favour vegetation so much as that philosopher asserted.

M. Rafn sowed barley in a mixture of mould, sand, and manganese, in order to see whether the oxygen gas would

* These experiments would be more conclusive had not the author added mould in the boxes into which he put the rubbish. It is well known that mould contains a great quantity of carbon, exactly in the state which renders it fit for the nutrition of vegetables.

M. A. Young, on the other hand, asserts, that plants grow exceedingly ill in charcoal: and this observation agrees more with the others, and with the reasoning, which induces us to believe that carbon must be dissolved to enter into combination with the other principles of vegetables. As plants grow exceedingly well in pure water till a certain period, it would appear that they ought to grow equally well in watered charcoal.

not

not be difengaged in fuch a manner as to produce fome effect on plants. At firft he obtained no effect; but having watered this box with diluted fulphuric acid, he remarked that the barley vifibly grew fafter in this box than in thofe not watered in the fame manner *.

Of all the mixtures which he tried for fowing, none appeared to him better than that of equal parts of charcoal, mould, and fand, moiftened with water filled with infufion animals, which may be eafily obtained by fteeping flax in the water deftined for that purpofe. He obferves, on this occafion, that, of all the fubftances he tried, flax is that which furnifhes the moft of thefe animalculæ. An incredible multitude of them are found in the water in which women dip their fingers when they are employed in fpinning. The water put into a veffel for that ufe in the morning, is found filled with them in the evening. The author afcribes to thefe fmall animals a much greater influence on vegetation than has hitherto been believed.

Haffenfratz relates, that he could not make plants vegetate well in fimple earths. The author afferts, that he had great fuccefs when he reared them in pure filex, quartzy fand thrice wafhed, fine fand from the fea-fhore, &c. But thefe plants continued ftunted and pale, and their roots were twice as long as the whole of the part above the earth. In charcoal, on the other hand, the parts were large and vigorous; they were of an exceedingly dark colour, and their roots were not a fixth part of the length of the plant itfelf †.

Coal-afhes, on which the German and Englifh farmers beftow fuch praife, deftroy the plants if the foil contains an eighth part of that admixture. The leaves become faded, as

* The fulphuric acid cold does not difengage the oxygen of the oxyd of manganefe: befides, according to the experiments of Ingenhous, this acid alone, in fmall quantity, feems to have the property of rendering vegetation more active.

† The firft refults are perfectly fimilar to thofe obtained by C. Haffenfratz. In regard to the fecond, they depend on the purity of the charcoal employed, which may contain wood undecompofed, and confequently difpofed to putrify, and to yield a liquid which may hold the carbon in folution.

if scorched, at the end of from fifteen to twenty days, and the plants themselves die at the end of four or five weeks.

No seed germinates in oil. A single grain of common salt in two hundred grains of water is sufficient to retard the vegetation of plants, and may even kill them if they are watered with that saline liquor *.

Shavings of horn, next to infusion animals, are the most favourable to vegetation: charcoal holds the third rank.

V. *On the Assaying of Iron Ores and Iron-Stones by Fusion.* *By Mr.* DAVID MUSHET, *of the Clyde Iron Works* †.

IT will easily be conceived, from the mode of operation which I have adopted, that, in order to procure accurate results, the proportion of flux must be varied according to the mixtures in the iron-stones or ores; and that no universal solvent can be used as capable of assaying *all* ores.

As the gradation of mixtures in the ores is almost imperceptible, there are, in fact, no fixed limits by which Nature has distinguished the various classes: we find all the varieties diminishing their predominant earth, and assuming, in equal proportions, those of each other, thus constituting the class of equalised mixtures; yet, here, the variety of combination ceases not, the predominating earth gradually becomes the minor part of the mixture, and that which only held a second rank, as to quantity, is now the chief component earth; the permutation goes round, till the earth, which existed in the most sparing quantity, now predominates to excess.

* C. Sylvestore obtained a result absolutely similar, by employing marine salt as manure.

† The present is a part of the communication from Mr. Mushet which appeared in our Number for July last, (Vol. IV. p. 178,) but by an oversight of the Editor was omitted in its place. It contains the table of proportions alluded to in our Number for September last (Vol. IV. p. 380.) requisite for the obtaining from all the various iron-stones an accurate assay, and should have immediately followed Mr. Mushet's article given in our July number.

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In fuch an infinity of variation it is difficult to arrange the combinations of which thefe fubftances are capable. To derive the name of a clafs, or genus, from the predominancy of an earth, feems moft eligible; and to confider thofe as varieties of the fame clafs, which are altered by the proportion of the fecond and third mixtures. Again, each of thefe varieties are fufceptible of a multiplicity of modifications before an earth is fo far diminifhed as to give an afcendency to another, or before the third rank of proportion has affumed that of the fecond or firft. The fimple combination of the earths, and their degrees of predominancy, may be thus arranged:—

	1ft Variety,	2d Variety.	
Argillaceous iron-ftone	Iron -	- Iron	As thefe become varied, they form the clafs of equalifed mixture.
	Clay -	- Clay	
	Lime -	- Silex	
	Silex -	- Lime	
Calcareous iron-ftone	Iron -	- Iron	
	Lime -	- Lime	
	Clay -	- Silex	
	Silex -	- Clay	
Siliceous iron-ftone	Iron -	- Iron	
	Silex -	- Silex	
	Lime -	- Clay	
	Clay -	- Lime	

To affay any of thefe varieties, a flux peculiar to the nature of the mixture is neceffary; fo that the changes of proportion in the folvent ought to extend to feven, including the clafs of equalifed mixtures, in order that the precife fame quality of crude iron may be produced from all the varieties of iron-ftone. The modification of each variety will be found to be fufficiently accurate, if affayed by the flux peculiar to itfelf. The arrangement of the three claffes of ores into two varieties, each forming a diftinct ftage of combination, indicated by the predominancy of the firft and fecond earth, are, with the neutral clafs, fufficiently minute for any purpofe in the affay-furnace, and are fufficient to form an

accurate and extenfive knowledge of the analogy of thefe refults with thofe in the blaft-furnace.

Table *of* Proportions *of* Fluxes.

Let the earthy part of an argillaceous ore be compofed of clay 9, lime 6, fand 3 = 18.

To affay	4 troy ounces of this ore		-	or	1920 grains,
add	4	bottle glafs		—	1920
	3	chalk		—	1440
	$0\frac{1}{2}$	charcoal		—	240
	$11\frac{1}{2}$				5520

Let the fecond variety of argillaceous ores contain, clay 10, filex 7, lime 3 = 20.

In this cafe,	4 ounces troy of ore		-	or	1920 grains,
would require	4	bottle glafs		—	1920
	4	chalk		—	1920
	$0\frac{3}{4}$	charcoal		—	360
	$12\frac{3}{4}$				6120

Let the firft variety of the calcareous genus of iron-ftone be fuppofed to contain, of earthy mixtures, lime 14, clay 6, filex 4 = 24.

When this iron-ftone is to be affayed,

to	4 ounces troy		- -	or	1920 grains,
add	5	bottle glafs	-	—	2400
	$1\frac{1}{2}$	chalk	—	—	720
	$0\frac{3}{4}$	charcoal	—	—	360
	$11\frac{1}{4}$				5400

Again, let the fecond variety of the calcareous genus be fuppofed to contain, lime 10, fand 6, clay 4 = 20.

I would add to	4 troy ounces		-	or	1920 grains,
	4	bottle glafs		—	1920
	2	chalk		—	960
	$0\frac{1}{2}$	charcoal		—	240
	$10\frac{1}{2}$				5040

Let

Let the firſt variety of filiceous ores be ſuppoſed to contain, ſilex 12, clay 8, lime 5 = 25.

For an aſſay of	4 troy ozs. of ore		-	or	1920 grains,
add	4	——	chalk	—	1920
	3	——	bottle glaſs	—	1440
	$0\frac{3}{4}$	——	charcoal	—	360
	$11\frac{3}{4}$				5640

And, laſtly, let the ſecond variety of this genus of iron-ſtone be ſuppoſed to contain, ſilex 10, lime 7, clay 5 = 22.

To	4 troy ounces of the ore		-	or	1920 grains,
add	$3\frac{1}{2}$	——	chalk	—	1680
	3	——	bottle glaſs	—	1440
	$0\frac{3}{4}$	——	charcoal	—	360
	$11\frac{1}{4}$				5400

Claſs of equaliſed mixtures, compoſed of, clay 7, lime 7, ſilex 7 = 21.

To aſſay of this ore	4 troy ounces		-	or	1920 grains,
add	$3\frac{1}{2}$	——	bottle glaſs	—	1680
	$2\frac{1}{2}$	——	chalk	—	1200
	$0\frac{1}{2}$	——	charcoal	—	240
	$10\frac{1}{2}$				5040

VI. *On the real Origin of that Reſin known under the Name of Sandarac, and that of Gum Arabic.* By M. Schousboe*.

GUM Sandarac is an article of trade brought from the ſouthern provinces of the kingdom of Morocco. About ſix or ſeven hundred quintals of it are exported every year from Santa Cruz, Mogador, and Saffy. In the language of the country it is called *El graſſa*. The tree which produces it is a *Thuia*, found alſo by M. Vahl in the kingdom of Tunis.

* From a Daniſh Journal, entitled, *The Phyſical, Medical, and Economical Library*, Part III. 1799.

Tunis *. It was made known feveral years ago by Dr. Shaw, who named it, *Cypreffus fructu quadrivalvi, Equiseti inftar articulatis*; but neither of thefe learned men was acquainted with the economical ufe of this tree; probably becaufe, being not common in the northern part of Barbary, the inhabitants find little advantage in collecting the refin which exudes from it. This refin hitherto has been afcribed to the *Juniperus communis*, *Juniperus Lycia*, or the Cedar of Lebanon, without reflecting that the *Juniperus communis* does not grow in Africa, and Sandarac feems to belong exclufively to that part of the world. M. Schoufboe, who faw the fpecies of *Thuia* in queftion, fays that it does not rife to more than the height of twenty or thirty feet at moft, and that the diameter of its trunk does not exceed ten or twelve inches. It diftinguifhes itfelf, on the firft view, from the two other fpecies of the fame genus, cultivated in gardens, by having a very diftinct trunk, and the figure of a real tree; whereas in the latter the branches rife from the root, which gives them the appearance rather of bufhes. Its branches alfo are more articulated and brittle. Its flowers, which are not very apparent, fhew themfelves in April; and the fruit, which are of a fpherical form, ripen in September. When a branch of this tree is held to the light, it appears to be interfperfed with a multitude of tranfparent veficles which contain the refin. When thefe veficles burft in the fummer months, a refinous juice exudes from the trunk and branches, as is the cafe in other coniferous trees. This refin is the Sandarac, which is collected by the inhabitants of the country, and carried to the ports, from which it is tranfported to Europe. It is employed in making fome kinds of fealing-wax, and in different forts of varnifh. In 1793 a hundred weight of it coft in Morocco from 13 to 13½ piaftres, which make about 3*l.* 5*s.* to 3*l.* 7*s.* and 6*d.* fterling. The duty on exportation was about 7*s.* 6*d.* fterling *per* quintal.

Sandarac, to be good, muft be of a bright-yellow colour, pure and tranfparent. It is an article very difficult to be

* A complete defcription of it, with a good figure, may be found in his work entitled, *Symbol. Botan* Part II. p. 96. Plate XLVIII. under the name *Thuia* articulata.

adulterated.

adulterated. Care, however, muſt be taken that the Moors do not mix with it too much ſand. It is probable that a tree of the ſame kind produces the gum ſandarac of Senegal, which is exported in pretty conſiderable quantities.

Another article of commerce in which the kingdom of Morocco participates with Senegal, is that gum called Gum Arabic, known by the name of *Al leilk*. The tree which produces it grows only in the ſouthern provinces of that ſtate. The quantity of this article exported to the different parts of Europe from the ports of Morocco may amount to about eight or nine quintals. M. Schouſboe ſays, that this tree is the *Mimoſa nilotica*, named in the country *Al thlah*; but this is no reaſon why the ſame kind of gum ſhould not be collected in the more ſouthern countries of Africa from the *Mimoſa Senegal*, and even from other trees of the ſame ſpecies, as we are told by various authors. In Barbary the people make a difference between the gum of Senegal and that of the country. The former is preferred on account of its purity, whiteneſs, and tranſparency, which in general are the properties ſought for in this article.

The gum which I collected myſelf in the province of Mogador, ſays M. Schouſboe, exudes from the trunk and branches of the tree, in the ſame manner as that of our fruit-trees. It is in round lumps, of the ſize of a hazle-nut, or rather that of a walnut. Theſe lumps, indeed, by becoming united to each other, form maſſes ſometimes of the ſize of the fiſt, or even of the head; but this only happens in conſequence of the adheſion which takes place between the pieces of gum, when ſtill freſh, after they have been detached from the tree, and chiefly at that part which was attached to the bark, where the reſinous juice has not had time to harden. If earth, ſmall ſtones, and other foreign bodies, are ſometimes found in theſe maſſes, it ariſes from fraud; and M. Schouſboe ſuſpects that this circumſtance has given riſe to the opinion of the gum being found at the bottom of the tree, and that it exudes from the roots, which, as he thinks, is void of foundation. Were this the caſe, it appears to him that, beſides the earth and ſand with which the gum is accidentally dirtied, balls of theſe matters ought to be found

in the inſide, and even ſo mixed with the mucilaginous ſubſtance that it would be impoſſible ever to purify it completely; while, on the other hand, the gum which comes from Senegal is ſtill purer than that of Barbary.

M. Schouſboe, however, obſerves, that the ſandarac and gum exported from the port of Saffy have a brown or reddiſh colour; but he aſcribes this colour to the quantity of the red oxyd of iron mixed with the ſoil of the province of Abda, where this port is ſituated. This oxyd communicates its colour even to the whiteſt wool; and the inhabitants of that province may be diſtinguiſhed by the reddiſh tint of their clothes, which cannot be entirely deſtroyed by any proceſs. In the months of July and Auguſt, when heavy dews fall, the gum loſes a great deal of its tranſparency, as well as of its other good qualities. A hundred weight of this ſubſtance coſt at Mogador, in 1793, about 2 *l.* ſterling, without including about 4 *s.* cuſtom-houſe duty. The gum does not appear to be employed by the inhabitants of Morocco for any purpoſe whatever: the whole of what they collect is ſold to the different commercial nations of Europe.

VII. *On the Peſtilential Diſeaſes which, at different times, appeared in the Athenian, Carthagian and Roman Armies, in the Neighbourhood of Syracuſe. By the late* E. H. Smith, *Phyſician* *.

Section I.

Syracuse, the moſt beautiful of all the cities built by the Greeks, was founded by Archias, a Corinthian, of the race of Hercules †. He firſt expelled the natives from Ortygia, where he commenced the city, which was afterwards extended to the neighbouring continent. This place, ſo celebrated in ancient hiſtory, the birth-place of Archimedes, and theatre of many memorable tranſactions, now reduced to a miſerable town, of inferior conſequence even in Sicily, is ſituated in north latitude 37° 5′, a little above Cape Paſſara (the

* From the *American Medical Repoſitory*, Vol. II. No. 4.

† Cicero in Ver. Act. II. Lib. iv. § 117. Thucydides, B. vi.

(the ancient Pachynum), the fouth-weftern extremity of the ifland of Sicily.

The climate of Sicily, in general, is reprefented as favourable to health *; and, notwithftanding its infular fituation, more analogous, in the qualities of temperature and humidity, to the hilly, than to the Atlantic divifions of South-Carolina and Georgia. The winter is remarkably mild: with the exception of a few days, it equals the fineft fpring weather in the North of Europe †, and the Eaftern States of America; and the fhade is found pleafant, in the middle of the day, even in the month of January ‡. At Syracufe, in particular, the feafon is fo little affected by the feverities of cold and tempeft, that, during its whole courfe, and in its moft boifterous ftate, the fky is never totally obfcured for a fingle day §. Yet in this climate did Verres, the voluptuous prætor of Sicily, that monfter of atrocity and lafcivioufnefs, fo entirely feclude himfelf from all impact and influence of the atmofphere, as only to learn the approach of fpring by obferving the dew gliften on the verdure which furrounded his palace ||. As the feafon advances, the heat rapidly increafes, till, in the fummer, it is no longer to be endured by ftrangers, and exertion, as well as enjoyment, is temporarily fufpended. In autumn, the frequent rains, which are common throughout this feafon, and the heats of the middle of the day, contrafted with the extreme chillinefs of the evenings, render it lefs pleafant and falubrious than any other part of the year ¶. It is to the exceffive fultrinefs of the fummers that a late fenfible writer ** afcribes the imperfection of many,

* Eenige Berichten, &c. *i. e.* Some account of the Pruffian, Auftrian, and Sicilian monarchies, &c. See Monthly Review Enlarged, Vol. XIV. p. 491.

† Swinburne's Travels in the Two Sicilies, Vol. II. § 49. Dubl. edit.

‡ Keyfler's Travels, Vol. III. p. 33.—Tranflation.

§ Cicero in Ver. Act. II. Lib. v. § 26.

|| Cicero in Ver. Act. II. Lib. v. § 12.

¶ Travels in Sicily and Malta, by M. De Non, p. 307, 333, and 383.—Tranflation.

** Eenige Berichten, &c.—" The foil is exceedingly fertile; but, from the great heat of the climate, many of our moft nourifhing and refrefhing vegetables

many, and the total want of others, of the most nutritious and refreshing vegetables of the northern climates, in despite of that fertility of soil which, from time immemorial, has conferred on Sicily the appellation of the granary of Europe.

But, notwithstanding the general pleasantness and healthfulness of this island, the concurrent testimony of ancient and modern writers evinces the noxious condition of particular places. "The least stagnant water is sufficient, in the heats of summer, to poison the atmosphere: its effects on the countenances of the poor people, who live in its vicinity, are very evident; and a stranger, who travels through the island in this season, ought to avoid ever passing a night near them *. As soon as the sun enters the Lion, this country becomes the house of death: fevers, of the most malignant kind, seize upon the imprudent or unfortunate wretch that spends a night near them (ponds and marshes); and few escape with life, when attacked by so virulent a disorder †." Instructed, probably, by experience of the calamities consequent on a near residence to marshes and stagnant waters, the inhabitants in various parts of Sicily have built their towns on adjoining eminences ‡. To avoid the dangers which beset the stranger in journeying through this country in the sultry and autumnal seasons, was probably one of the motives of the Roman prætors for performing the tour of the island in the time of harvest §. By a neglect of similar prudence, the celebrated M. De Non became affected with a violent fever and ague, after an incautious exposure to the heat of the sun and the chill of the evening, in an unwholesome part of the country, and in the month of September ‖.

From this brief account of the climate of Sicily, it will be evident, that, how favourable soever it may be esteemed, in general, to the health and longevity of the natives, and how

vegetables will not thrive in it. Currants, raspberries, and gooseberries, are unknown to the natives; and foreigners, who have attempted to cultivate them, have never been able to succeed."

* Eenige Berichten, &c.

† Swinburne's Travels, Vol. II. § 49. Dublin edit.

‡ Swinburne's Travels, Vol. II. p. 300. Dublin edit.

§ Cicero in Ver. Act. II. Lib. v. § 80.

‖ De Non's Travels, p. 383.—Translation.

 pleasant

pleafant foever many parts of it deferve to be confidered as a winter and even vernal refidence for ftrangers; yet, in other feafons, and particularly for vifitors from colder countries, it muft be in moft parts unfalutary, in others certainly fatal, and, in fome, not to be long continued in, with impunity, by the inhabitants themfelves. And, as this remark refers wholly to ordinary years and circumftances, it will afford fome ground for eftimating the effects of one of the moft unhealthy fituations in the country, in the fickly feafon of the year, with a conftitution of the atmofphere favourable to epidemic difeafes, on an army of foreigners, tumultuous and ill-accommodated, worn down with fatigue, or finking in defpondency.—With this reflection impreffed on the mind, we may now proceed to a defcription of the city of Syracufe, and the country immediately adjacent, as they exifted in ancient times, occafionally fupplying illuftrations from the accounts of modern travellers.

The once beautiful city of Syracufe confifted of five principal divifions, traces of all which are ftill difcernible. 1. The ifle Ortygia, Nafos, or Nafon: 2. Achradina: 3. Tyche, or Tycha: 4. Neapolis, or the new city: and, 5. Epipolæ *.

Ortygia, enclofed by the two ports—the great port on the weft and the little port on the eaft, and connected with the continent by a bridge—is of an oblong form, and about two miles in circumference †. Here the original fettlement commenced, and this was always confidered as the wealthieft and moft defirable part of Syracufe. It was the ancient refidence of the kings; and, in the time of Cicero, ftill contained the houfe of Hiero. It was ornamented with various public buildings, particularly the temples of Diana and Minerva; and, in part, watered by the poetic fountain of Arethufa. As the population increafed, the found, which divided Ortygia from the Continent, was filled up, and the ifle converted into a peninfula ‡. Carlos III. of Spain removed the earth by which they were united; and Ortygia is again an ifland, and connected with the Continent, as formerly, by a bridge §.

* Cicero in Ver. Act. II. Lib. iv. § 118. De Non, p. 304. et fequent. Swinburne, Vol. II. p. 309.
† De Non. Swinburne.
‡ De Non. Swinburne.
§ Brydone's Tour, Letter xiii.

The

The ſituation of Ortygia is important, for it commands both the ports; and though ſuppoſed itſelf to be commanded by Achradina, yet, while that quarter of the city remained in the poſſeſſion of the Syracuſans, together with the iſle, and the oppoſite promontory of Plemmyrium, the town was nearly inacceſſible to a naval force *. At preſent Ortygia is the only city. To this ſtate it was reduced by the Muſſulmen. It is ſtrongly defended towards the land, weakly towards the ſea-ſide. Its quay is ſmall; its ſtreets narrow, winding, and wretchedly built; and its population does not exceed 18,000 perſons †. The fountain of Arethuſa, after repeated changes of ſituation ‡, is ſtill diſcoverable in the weſt part of the iſle; but its beauty and its honours have fled with the mythology to which it is indebted for its fame.

Achradina. The quarter of Achradina, at the period to which this eſſay has particular reference, was the moſt ſpacious, well built, and ſtrongly fortified part of the city. It extends over two conſiderable levels, divided by a natural wall of calcareous rocks; the one as elevated as Tyche; the other, and more conſiderable, on a plane with Ortygia, and thence conferring on this quarter the character of the loweſt diviſion of Syracuſe. The eaſtern part was the moſt commodious, and not leſs extenſive than the modern Paris. The whole was adorned with a large forum, a beautiful portico, prytaneum, curia, and the temple of Jupiter Olympus; bordered on three ſides by the ſea, by the great port on the weſt, the leſſer on the ſouth, and the port of Trogilus on the eaſt; and, on all ſides, nearly impregnable. The rocks of this quarter of Syracuſe, which are formed by marine depoſitions, poſſeſs the ſingular property of diſſipating or abſorbing the moiſture of dead bodies ſo ſpeedily, that they are preſerved in vaults excavated for the purpoſe, in their proper form and habiliments. Achradina is remarkably fertile, and naturally adapted for bringing to perfection every tropical production §.

* De Non. Swinburne.

† Swinburne, Vol. II. p. 311, 312. De Non, p. 304.

‡ De Non, p. 307.

§ Cicero ut antea. De Non, p. 321, et ſequent. Swinburne, Vol. I. p. 313, 314, 315. Rollin's Rom. Hiſt. Vol. V. p. 204.

Tyche,

Tyche, or the third city, extended northerly from Achradina to Epipolæ; commencing at the bottom of the port of Trogilus, which forms its ſouth-eaſtern boundary. Eaſt, it was defended by a ſtrong wall; and a wall divided it, on the weſt, from Neapolis. The famous gate of Hexapylon was on the eaſtern wall, and oppoſite to the little town of Leon. Tyche was ornamented by the Gymnaſium; and, though now deſolate, was once filled with inhabitants. It is very elevated, riſing in rapid gradation from the wall of Achradina *.

Neapolis ran nearly parallel, and in the ſame direction with Tyche; terminating on the north, with that quarter, at Epipolæ. On the eaſt, ſouth, and weſt, it was equally protected by a wall. The upper and northern part of Neapolis, as of Tyche, was elevated; its ſouth-weſtern extremity was conſiderably lower. A noble theatre, and the temples of Ceres and Bacchus, were its moſt diſtinguiſhed ornaments †.

Epipolæ was originally a piece of high ground without the city, and afterwards ſo little inhabited that it is not mentioned by Cicero in his deſcription of Syracuſe. The moſt elevated ſituation, and commanding Tyche and Neapolis, it was judiciouſly incloſed by Dionyſius I. who ſurrounded it with a wall of near four miles in extent. Its additional defence was the fortreſs of Labdalon, at its bottom, on the eaſt; and that of Euryalus, at its top, on the north ‡.

The whole extent of the city of Syracuſe, according to Strabo, whoſe account is verified by the concurrent teſtimony of Mr. Swinburne, was equal to twenty-two miles and four furlongs Engliſh meaſure §.

To this brief deſcription of the principal diviſions of this famous city, it is neceſſary to add a few remarks relative to its immediate vicinage, and the waters by which it is ſurrounded.

The *Great Harbour*, or Port, is about five miles in circumference, and forms, at its north-weſtern extremity, the

* Cicero. Rollin. De Non. Swinburne.

† Cicero. Rollin. De Non. Swinburne.

‡ Rollin. Swinburne. De Non.

§ Swinburne, Vol. II. p. 309.

Gulf

Gulf of Daſcon. Lower down, and oppoſite to Ortygia, it is contracted by the promontory and fort of Plemmyrium. The *Little Port*, or Portus Marmoreus, divides Ortygia from the Continent on the eaſt, and waſhes the ſouthern wall of Achradina. The *Bay of Thapſus* runs up behind Achradina on the eaſt, and forms, by the junction of its weſtern extremity with the city, the *Port of Trogilus* *.

At the diſtance of about two miles from Ortygia, and ſomewhat leſs than a mile and a half from Neapolis, (whoſe weſtern wall croſſed the low grounds through which it runs,) the river Anapus empties into the great harbour. This ſtream, which is only twenty-four feet wide, and twelve or fifteen deep at its mouth, flows, in a ſerpentine courſe, through a ſmall extent of country, which, though ſlightly elevated on its ſouth or ſouth-weſtern ſide, on the north and north-weſt conſiſts of an extenſive marſhy plain †.

Between the Anapus and the promontory of Plemmyrium was ſituated the little ſuburb of Olympia, ſurrounding the ſcite of the ancient temple of Jupiter Olympus, built upon an eminence, and bounded on either ſide by the vaſt Lyſimelian marſhes, extending from the head of the great harbour, half covered with water in the vernal months, and exhaling, under a vertical ſun, the moſt unwholeſome and pernicious vapours ‡.

The preceding detail, it is hoped, will neither be found altogether tireſome, nor foreign to the purpoſe of this eſſay. Some part of it might, indeed, have been omitted, as not directly eſſential to the main deſign of the paper; but there would have been leſs unity in the deſcription, and ſome portion of the ſubſequent narration might have been leſs perfectly comprehended,

* Rollin. Swinburne. De Non.

† Rollin. Swinburne. De Non. Thucydides, B. vi. and vii.

‡ Rollin. De Non. Swinburne. Watkin's Travels, Letter xxiv.

The reader will find great aſſiſtance in comprehending diſtinctly every part of the preceding deſcription, from conſulting the plan of Syracuſe in Rollin's Ancient Hiſtory, or that in his Roman Hiſtory, which, though inaccurate in ſome reſpects, I have reaſon to believe the moſt correct.

Sect.

SECT. II.—The peftilential difeafes prevailing in the neighbourhood of Syracufe, to which the prefent inquiry is limited, occurred at three diftinct periods: at the three fieges of that city, by the Athenians and their allies, under Nicias; by the Carthaginians, under Imilcon; and by the Romans, under Marcellus. It is exceedingly to be regretted, that, confidering the fingular mortality which diftinguifhed each of thefe plagues, the information concerning them is fo fcanty and general. But the precifion which remarkably characterifes that which remains, renders it of fufficient importance to deferve particular attention. It is the purpofe of the prefent Section to place before the reader, in a fuccinct narration, the circumftances which the hiftorians have preferved relative to each of thefe interefting events.

In the fecond year of the famous conteft between the Syracufans and the Athenians, of which Thucydides has left fo minute and fo touching an account, the Athenians, whofe principal encampments were on the eaft of Syracufe—at Leon, at Thapfus, at Labdalium, and on the port of Trogilus—undertook the conftruction of a wall, which extended to Epipolæ, and was defigned, after croffing the plain and the marfhes of the Anapus, to reach to the great harbour on the weft, and inclofe the city. From the completion of this enterprife they were prevented by the exertions of the Syracufans, who carried a divifion-wall from the city acrofs the marfh, and thus oppofed an infurmountable barrier to the progrefs of their befiegers. To promote his defign, of the fuccefs of which he now began to entertain doubts, and to enable him more effectually to annoy the enemy by water, the Athenian general fortified and tranfported his army to Plemmyrium. From this time the fortune of the Athenians changed: their fatigue was great and inceffant; every trifling fuccefs was fucceeded by fome fad reverfe; relief was diftant and uncertain; and they became a prey to defpondency, not a little heightened by a fenfe of the injuftice of their caufe, their exemplary humiliation, and the unexpected fuperiority of their antagonifts. Under thefe circumftances, in the autumnal feafon, and "encamped on marfhy and

 unwhole-

unwholesome ground *," a situation "always unhealthy for an army, and especially in this season of the year †," a pestilential disease shewed itself in the camp, extending with the progress of the season, and the continued exposure of the soldiery, or, as Plutarch expresses himself, by contagion; till, harassed by the enemy on one part, and worn down by fatigue and sickness on the other, the Athenians were driven to the sad necessity of attempting a forced and secret retreat, with the dereliction of their camp, their wounded, and their sick. The consummate eloquence of Thucydides alone is adequate to the description of this scene of horrors.

Of the numbers who perished by this pestilence, and of its particular symptoms, no record is transmitted down to us. The event of this siege is known. The Athenians were surrounded on their retreat, and defeated with immense destruction, and under the most melancholy circumstances of distress: their general, worthy of a better fate, was cruelly put to death; and the greater part of those who were made prisoners perished in Syracuse, the victims of diseases induced by excessive labour and unwholesome food. A few (in such honour was poesy among the ancients,) were emancipated by the recollection and recitation of even a single verse of the pathetic tragedies of Euripides ‡.

At a subsequent period, and during the war between the Carthaginians and Dionysius the elder, Himilco, or Imilcon, after several successful enterprises in other parts of Sicily, marched against Syracuse. He invaded it with an army of 300,000 foot and 3000 horse; while the Carthaginian fleet, of 200 ships, under the command of Mago, followed by 500 barks, entered the great port in a triumphal manner, and laden with the spoils of the ravaged cities of the island. Imilcon pitched his tent in the very temple of Jupiter, then standing at Olympia; and his army encamped around him.

* Smith's Thucydides, Vol. II. p. 236.

† Plutarch, art. Nicias.

‡ *Ibid.* – For the particular history of the siege of Syracuse, the reader is referred, generally, to Thucydides, B. vi. and vii.; and to the Life of Nicias, by Plutarch.

In this situation, "an eminence between two morasses, highly favourable for a camp, and for rendering it impregnable," he continued thirty days, laying waste the country on all sides, plundering the temples, and demolishing the tombs of the kings, and in vain offering battle to the Syracusans, who had not the courage to attack or oppose him. Nature, more powerful than themselves, arrested the progress of that fate which threatened to overwhelm them. "It was now in the midst of summer, and the heat this year was excessive." While Imilcon continued at Olympia, spreading devastation around him, and anticipated a signal revenge upon the hostile Syracusans, a pestilential malady shewed itself in his camp. It appeared first among his auxiliaries, the Africans, who, forced into the service by fear of the Carthaginians, whom they hated, were probably least carefully accommodated. From them it soon extended, by the increasing virulence and activity of the exciting cause, throughout the army. Neither care nor medicine afforded relief. At first the sick received some assistance from the well; but the pestilence and the mortality multiplied themselves so rapidly in every direction that this soon became impossible, nor were there men to be found for the performance of the rites of sepulture to the accumulating dead. "Violent dysenteries, raging fevers, burning entrails, and acute pains in every part of the body," were the usual symptoms of this terrible disease. Some were even seized with madness, and in their phrensy attacked and endeavoured to destroy all that were exposed to their assaults. Meantime the Syracusans, in health and safety in their elevated, dry, and airy city, watched the progress of the pestilence among their enemies, and, profiting by their forlorn condition, captured or involved in flames their mighty fleet; and, storming their camp, impregnable to the greatest force when defended by even a small body of healthy troops, defeated them with exemplary destruction. Thus beset, enfeebled by disease and humbled by defeat, the proud Imilcon, who, a short time before, held the conquest, not only of Syracuse, but of Sicily, too cheap and easy a victory for so formidable an host, was reduced to purchase his safety

 for

for 300 talents, and fled, leaving unburied the carcaſes of 150,000 of his ſoldiery *.

The hiſtory of the laſt of thoſe peſtilential diſeaſes, which are particularly ſelected for preſent conſideration, deſerves a longer and more attentive examination. Furniſhing excellent illuſtration of ſeveral important points in the hiſtory of theſe diſorders, the reader will doubtleſs excuſe ſuch preliminary narration as may conduce to place it more fully before him.

The Roman army, under the command of Marcellus, decamped from their ſtation at Leontium, the modern Lentini, and, arriving at Syracuſe, encamped at Olympia. After an unſucceſsful attempt at negociation, the city was beſieged in due form; and the attack commenced on Hexapylon by land, and on Achradina by water. But the Roman general was not ſucceſsful at Syracuſe, as he had been at Leontium: all his exertions were rendered nugatory by the talents of a ſingle citizen; and the genius of Archimedes triumphed over the power of Rome.

* Rollin's Ancient Hiſtory, Vol. I. p. 302, 30 , 04 De Non's Travels, p. 358, 359, &c. Diodorus Siculus, Lib. xiv. p. 279—295.

The Carthaginians ſeem to have been deſtined to owe the defeat of many of their moſt promiſing enterpriſes in Sicily to peſtilential diſeaſes. In a former part of the ſame war, whoſe unhappy termination has been deſcribed above, Hannibal, the predeceſſor of Imilcon, laid ſiege to the city of Agrigentum (Girgenti), in the ſouth-weſtern part of Sicily. For the purpoſe of raiſing a wall without, which ſhould overlook and command the city walls, he collected all the materials within his reach, and, among the reſt, deſtroyed, and converted to this uſe, the tombs ſtanding round a city very ancient and populous, and then containing 200,000 inhabitants. From the uncovering and diſturbing of ſo many dead bodies aroſe a terrible peſtilence, which carried off immenſe numbers of the Carthaginians, and the general himſelf. Afflicted at this dreadful mortality, the beſiegers attributed it, with the ſuperſtition of the age, to the vengeance of the gods, incenſed againſt them for violating the repoſe of the dead. The healthineſs of the ſituation, the ſeaſon, and the thorough appointment and ſupplies of the Carthaginian army, leave no room to doubt as to the real cauſe of the ſickneſs, which gradually diſappeared: but two remarks, of ſome importance, are ſuggeſted by it. 1. The folly of modern nations, eſpecially in warm climates, in ſuffering the interment of the dead within their cities. 2. The wiſdom of ſome ancient nations, in having a *dead*, as well as a *living* town.

Neceſſitated

Neceſſitated to defer the further operations of the ſiege, the conſul directed his arms againſt ſeveral other Sicilian cities, and ſtruck his enemies with terror by his brilliant achievements. About the ſame time the Carthaginian general, Himilco, arrived at Agrigentum, to the aid of the Sicilians, with an army of 25,000 foot, 3000 horſe, and twelve elephants. Hippocrates, one of the Syracuſan leaders, with a part of the Syracuſan troops, marched out to meet him; Epicides having been left, with the remainder, for the defence of the city. After ſome ſkirmiſhes with the Sicilian army, in which he was fortunate, Marcellus returned to Syracuſe, whither he was ſoon followed by Himilco and Hippocrates, who, having formed a junction, fixed their camp at the river Anapus, about eight miles from the city. Nothing of importance occurred while the armies lay near each other. The combined chiefs ſoon drew off their forces; the Carthaginian general took up his winter reſidence at Agrigentum, and the Sicilian at Murgantia. Marcellus, who had been for ſome time buſied in the interior, now again returning, appointed Criſpinus to the command of the ancient camp at Olympia, and built and fortified a camp at Leon, on the eaſtern ſide of Syracuſe, for himſelf.

The Romans commenced their operations early in the ſpring. Some of them having gained an entrance into Syracuſe in the night, by ſtratagem, the gate at Hexapylon was broken open, and Marcellus entering, ſecured poſſeſſion of Epipolæ before any effectual reſiſtance could be attempted by Epicides. Tyche and Neapolis ſurrendered at diſcretion; the fortreſs of Euryalus ſpeedily ſubmitted; and the conſul diſpoſed this part of his army againſt Achradina, in three places, hoping to carry it by attack, or ſubdue it by famine.

While theſe preparations were going forward on the part of the Romans, Himilco and Hippocrates ſuddenly returned to Syracuſe, and encamped on the great harbour. From hence they attacked Criſpinus in the ancient camp of the Romans, while Epicides, in concert, ſallied out from the city upon the poſts of Marcellus. Neither of theſe aſſaults ſucceeded. Criſpinus repulſed the allies, and purſued them triumphantly

umphantly to their ſtation, at the ſame time that Marcellus compelled Epicides to take refuge within the walls of Achradina. After this, both of the Roman commanders ſtrengthened their encampments.

Such was the progreſs and ſtate of the war when a peſtilence, common to both armies, appeared both in the camp of the allies and of Criſpinus, and naturally diverted their attention from hoſtile operations: for the autumnal ſeaſon, their unhealthy ſituation, and the heat, (much more intolerable without than in the city,) affected almoſt every perſon in either camp. At firſt they became ſick, and died, ſimply from the effects of the ſeaſon and the unhealthineſs of their ſituation: afterwards, ſays the hiſtorian, the care and contact of the ſick ſpread the diſeaſe; ſo that thoſe who were attacked with it periſhed, neglected and forſaken, or their attendants fell victims to their humanity. Deaths and funerals paſſed before the view in rapid ſucceſſion, and day and night reſounded with lamentations. At length theſe ſcenes of calamity became ſo familiar, that they not only neither wept, nor lamented the dead, but even ceaſed to remove, or yet to inter them. Their lifeleſs bodies lay extended in heaps, in the very ſight of thoſe who expected a ſimilar fate; while the dead infected the ſick, and the ſick thoſe who were in health, as well with fear as with the corruption and peſtiferous exhalations from the bodies: ſo that, impatient of life, and deſirous rather to fall by the ſword, ſome ſingly invaded the ſtations of the enemy. The plague raged with far greater violence in the Carthaginian than in the Roman camp; for the Romans, from long reſidence in the vicinity of Syracuſe, had become accuſtomed to the air and water. As ſoon as the Sicilian auxiliaries of the Carthaginians perceived that the diſeaſe ſpread from the inſalubrity of the place, they betook themſelves to their neareſt cities; but the Carthaginians, admitted into no city, together with their generals, Himilco and Hippocrates, totally periſhed. Marcellus, obſerving how faſt the diſorder increaſed, drew the remainder of his troops into the city, where, under ſhelter, and in the ſhade of the houſes, they regained health and vigour, notwithſtanding

withſtanding many of the Roman army were carried off by the ſame peſtilence *.

Thus the Lyſimilian marſhes triumphed over a third army; and a number not leſs than 30,000 were added to thoſe who had fallen of the ſoldiery of Nicias and of Imilcon. The practical conſideration of theſe events belongs to the ſucceeding Section.

SECT. III.—The ſimplicity and uniformity of the preceding ſtatements and narrative, preclude the neceſſity for laborious inveſtigation and argument. The deductions to be made are unavoidably ſimple and uniform; but their force and tendency may be more diſtinctly perceived from a methodical arrangement.

1. Climate and ſeaſon.—The climate of Sicily in general, and particularly of the city of Syracuſe, is remarkably pleaſant during a conſiderable part of the year. But the ſummers are exceſſively ſultry; and this heat, operating on marſhy ſituations and ſtagnant waters, never fails to engender malignant fevers. To theſe they are peculiarly liable in the ſummer and autumnal months. The peſtilential diſeaſes which proved ſo fatal to the armies of Nicias and Imilcon, and to thoſe of the Carthaginians and the Romans, occurred either in the autumn or in the height of ſummer; and, in every inſtance, in the immediate vicinity of Syracuſe.

2. Situation.—The camp of Nicias was near or at Plemmyrium; of Imilcon, at Olympia; of Criſpinus, at Olympia alſo; and of Himilco and Hippocrates, at the head of the great harbour. The whole of this territory is a vaſt marſh, but partially covered with water even in ſpring, extremely offenſive in ſummer, and noted, from time immemorial, for its inſalubrity.

3. Condition of the armies.—The Athenian army was in conſtant action, and conſtantly unſucceſsful. The army of Imilcon was in perpetual movement, but fortunate, and full of courage. The adverſe armies, at the third ſiege, were in a ſtate of inaction. Theſe are certainly conſiderable differences:

* Tit. Liv. typis Barbore, 1775. Tom. III. Lib. xxiv. & xxv. p. 303—403.

ferences: why should they not modify the event? It may be doubted whether any moral cause would be sufficient to protect, for a long period, an unaccustomed resident in a marshy situation from the usual consequences. Fatigue and despondency are certainly favourable to the access of disease; and inaction, particularly in camps, is generally admitted to be scarcely less so. The uncommon heat of the year of Imilcon's irruption into Sicily, may be a satisfactory explanation why his successful troops were so readily affected by the unwholesomeness of his camp.

4. Progress of the disease.—The sick, in the army of Nicias, were not numerous at first, but the number gradually increased. This is attributed, by Plutarch, to contagion; but Thucydides, a cotemporary and more sagacious historian, expresses no such opinion, nor does it seem probable. The growing desperation of the Athenian affairs, and the tendency of the season to augment the extent and heighten the virulence of marsh exhalations, sufficiently account for the increasing predisposition to sickness, and for its more general prevalence in their army.

The same reasoning will apply, generally, to the case of Imilcon's army; but there were, in this instance, additional causes for the production of such immense mortality. The commencement of this pestilence was among his African auxiliaries. The circumstances of their impressment into the service, and of the contempt in which they were held by the Carthaginians, render it probable that less care was taken to accommodate them than the others. Some peculiar habits of body, modes of life, or varieties of native climate, might influence this event; or they might have been previously subjected to greater fatigue. Be this as it may, the sickness soon became universal, and so mortal that the dead lay unburied. In a situation like this, there is no reason to believe that much attention was at any time paid to the necessary duties of cleanliness. When the sick could find no attendants, and the dead none to inter them, it requires but little sagacity to infer that no means were used for the removal of excrementitious matters. And as a large proportion of this army were afflicted with dysentery, and the number of pu-

 trefying

trefying carcafes muft have been immenfe, and as the limits of the camp were not very extenfive, and they were now forely preffed by a vigorous enemy, incapable themfelves of refiftance, and fuddenly precipitated from triumph to inevitable defeat and fhame, we fhall find no difficulty in affigning caufes fufficient and fatisfactory for the extraordinary fatality of this peftilence.

The third plague commenced at the fame time in both the Carthaginian and Roman camps. At firft, as in the former inftance, the fick were attended to; but the number foon became fo great as to render this impoffible. In this cafe, as in the other, the dead were left unburied, and to putrefy; and in this cafe, as in that, there is no reafon to doubt but that the caufes and fatality of the difeafes were in confequence exceedingly augmented. It may reafonably be doubted, however, whether the hiftorian is accurate in afcribing any part of the fpread of the peftilence to contact of the fick. The belief of this doctrine has been of long duration and extenfive prevalence; but late obfervations do not feem to countenance its validity. To eftablifh it, would inevitably annihilate a diftinction much infifted on by the advocates for the importation of peftilential difeafes into the United States; but this argument is not wanted for that purpofe. It is more probable that the peftilence, in this inftance, as in that preceding, was rendered more general by the increafing quantity and concentration of marfh effluvia; by the excrementitious matters in the camps, fuffered to affume, unremoved, a putrefcent action; and by noxious exhalations from putrefying bodies. There are no facts which authorife the opinion that a fpecific matter emanates from any part of the body, during peftilential or any other fevers properly fo called, capable of generating a fimilar or a febrile difeafe. The moft that we may venture to infer from all the facts known to us, is, that the perfpirable matter, like any other animal fubftance, when feparated from the living body, or deprived of life, is liable, under certain circumftances of temperature and moifture, to undergo a putrefactive fermentation, or to go through fuch changes as fhall adapt it for the production of febrile difeafes; and this,

as well when suffered to lie on the skin, as when elsewhere, or otherwise disposed of. With these limitations, and in this view of the question, there need be no objection to the testimony of Livy; nor have we any ground, from a knowledge of the symptoms of the plague he describes, to oppose or fortify his narration.

5. Mortality of the pestilence.—Of the mortality of the sickness which affected the army under Nicias, we only know that it was considerable. Our knowledge is not much more definite respecting the number of the Carthaginians who perished of the army of Imilcon. There is reason, however, to believe that it far exceeded that of the Athenians, and probably it was not less than 100,000 men. For it must be recollected, that many had been regularly interred in the first of the plague, and that all the sick and wounded were left behind when their general ingloriously fled from Sicily. Nor is it probable that a number greatly exceeding 50,000 fell in the battle which preceded his flight. In the last case, our computation may be nearer the truth. The army with which Himilco invaded Syracuse amounted to 28,000, exclusive of his Sicilian auxiliaries, and of the troops from that city which joined him under the command of Hippocrates. The defeat of this last, by Marcellus, the preceding year; the secession of the Sicilians; and his own losses in the various actions in which he had been previously engaged, had no doubt reduced his army below its original strength. But, when it is remembered that all this army perished, and that a less, but still a great number of the Roman army also were destroyed by the plague, there is reason to estimate the whole loss as exceeding 30,000. The entire number of men sacrificed to this unhealthy situation, in three successive periods, could not be less than 150,000: a most melancholy illustration of the influence of climate, season, and soil, on the health of mankind; and an example of the activity of local causes in producing disease, compared with which, all the yellow fevers of the United States, whether originating here, or imported from abroad, scarcely deserve to be remembered.

6. Symptoms and nature of the pestilence.—On this point our information is less complete than on any other.

 The

The ſickneſs in the Athenian army is ſimply called a fever, ſaid to be contagious by Plutarch, but probably not ſo. Livy deſcribes no ſymptoms of the plague recorded by him. Diodorus Siculus is ſomewhat more particular; but only ſo far as to mark the variety of form in which the ſoldiers of Imilcon were attacked by the diſeaſe. In ſome, in the ſhape of enteritis, or inflammation of the bowels; in others, of dyſentery, ſometimes invading as a violent fever, with acute pains in every part of the body, and ſometimes with madneſs or delirium. Yet, even theſe ſcanty particulars are of importance, as they ſerve to identify the diſeaſe, and to proclaim its ſtrict affinity to thoſe which are the common offspring of ſuch ſituations as that in which the army were encamped, and thoſe which have ſpread ſo much apprehenſion, and excited ſo much diſcuſſion in our own country.

7. Ceſſation of the peſtilence.—Another circumſtance which characteriſes the diſeaſe under conſideration, is the manner in which it was extinguiſhed. So long as Nicias remained in his camp, he ſaw his men conſtantly dying around him by ſickneſs. His removal, notwithſtanding the ſubſequent calamities which befel him, appears to have thoroughly delivered his army from fevers. The ſurvivors were employed as ſlaves in Syracuſe; which could ſcarcely have happened had they been ſick, or had the Syracuſans dreaded the introduction of a contagious diſeaſe by their means.—Imilcon preſerved the reſidue of his army only by flight: there was no other hope for their ſafety; and, after his return to Carthage, he exclaimed, in the bitterneſs of his grief, that the plague, not the enemy, had conquered him! But neither then nor before did the dry and airy city of Syracuſe ſuffer from any ſickneſs; nor did the fear of contagion prevent the Syracuſans from repeatedly attacking the Athenian and Carthaginian camps. Their minds were probably unwarped by the bias of ſyſtem; and they diſcerned, in the poſition of their enemies, the true cauſe of their misfortunes.—The facts related by Livy are ſingularly preciſe and important. In the firſt place, neither the citizens of Syracuſe, nor the army of Epicides, which had poſſeſſion of Ortygia and Achradina,

nor that part of the Roman army which, under the command of Marcellus, was stationed in the elevated quarters of Tyche, Neapolis, and Epipolæ, were in the least affected by the plague. But this calamity was limited to the Romans under Crispinus, in the ancient camp at Olympia; and to the allies, who were encamped at the head of the great port. Crispinus and his troops, who had lived at Olympia a considerable part of the two preceding years, or at least had remained in the vicinity of Syracuse, and had become in a degree habituated to the air and water, suffered less severely than their enemies; and, when the sickness became general among them, recruited, and ceased to be taken sick, in consequence of a removal to the high grounds of the city, occupied by their countrymen. The natives of the island, seeing the danger to which they were exposed, took refuge in their own cities, and escaped the disease; but the Carthaginians, without any place of refuge, and entirely unaccustomed to their situation, totally perished.

8. On the whole, then, it appears that the mortality, in every instance, occurring in the armies near Syracuse, originated entirely from local causes: that there is no reason to suspect that this mortality was heightened by contagion, in the usual acceptation of that term: that the symptoms, so far as we have any account of them, were similar to those which occur, under similar circumstances, in the East and West Indies, in our own country, and in every part of the world: and that, as no difference is recorded as existing between these great epidemics or endemics before and after the causes were increased by animal putrefaction, the advocates for the foreign derivation of pestilential diseases must relinquish the distinction between fevers from animal and vegetable putrefaction; or, if they maintain the contagious quality of the former, must admit, on equal evidence, the same quality as characterising the latter. To this it may be added, that, as it was the universal practice of the ancients to designate any and every wide-wasting disease by the name of plague, without any special reference to its peculiar symptoms, so there is an equal propriety in our conferring the same appellation on our

our dyſenteries and fevers; nor do theſe leſs deſerve the title than the fevers of Smyrna, Cairo, and Conſtantinople, which reſemble them in their origin, their varieties of prevalence and degree, their appearance and diſappearance, and which would, in all probability, yield to the ſame regimen and remedies.

VIII. *Obſervations on the Art of Scowering different Kinds of Stuff.* *By* C. CHAPTAL*.

THIS art ſuppoſes, 1ſt, a knowledge of the different ſubſtances capable of ſtaining any kind of cloth; 2d, of the ſubſtances to which recourſe muſt be had in order to make thoſe depoſited on the ſtuff to diſappear; 3d, a knowledge of the effects produced on colours by thoſe re-agents which it may be neceſſary to employ to deſtroy ſtains; 4th, a knowledge of the manner in which the cloth is affected by thoſe re-agents; 5th, of the art of reſtoring a colour changed or faded.—Of thoſe bodies which occaſion ſpots on different kinds of cloth, ſome are eaſily diſtinguiſhed by their appearance, ſuch as greaſy ſubſtances; but others have more complex effects, ſuch as acids, alkalies, perſpired matter, fruits, urine, &c. Acids redden black, fawn, violet and puce-colour, and every ſhade communicated with orchilla-weed, iron, aſtringents, and every blue except indigo and pruſſian blue. They render the yellows paler, except that of arnatto, which they change into orange.

Alkalies change to violet the reds produced by Brazil wood, logwood, and cochineal. They render the greens on woollen cloth yellowiſh, make yellow browniſh, and change the yellow produced by arnatto to aurora. Perſpired matter produces the ſame effects as alkalies.

When the ſpots are produced by ſimple bodies on ſtuffs, it is eaſy to remove them by the means already known. Greaſy ſubſtances are removed by alkalies, ſoaps, the yolk of eggs, fat earths; oxyds of iron, by the nitric and oxalic acids; acids by alkalies, and reciprocally. Stains of fruit on white

* From the *Bulletin des Sciences*, Vol. II. No. 4.

ſtuffs

ſtuffs may be removed by the ſulphurous acid, and ſtill better by the oxygenated muriatic acid. But when the ſpots are of a complex kind, it will be neceſſary to employ ſeveral means in ſucceſſion. Thus: to deſtroy the ſtain of coom from carriage-wheels, after the greaſe has been diſſolved the oxyd of iron may be removed by the oxalic-acid.

As colours are often changed by re-agents, it will be neceſſary, in order to reſtore them, that the ſcowerer ſhould poſſeſs a thorough knowledge of the art of dyeing, and how to modify the means according to circumſtances. This becomes the more difficult when it is neceſſary to re-produce a colour ſimilar to that of the reſt of the ſtuff, to apply that colour only in one place, and often to reſtore the mordant by which it was fixed, and which has been deſtroyed, or even the firſt tint which gave the colour its intenſity. It may be readily conceived that the means to be employed muſt depend on the nature of the colour and the ingredients by which it was produced; for it is known that the ſame colour may be obtained from very different bodies. Thus, after an alkali has been employed to deſtroy an acid ſpot on browns, violets, blues, poppies, &c. the yellow ſpot which remains may be made to diſappear by a ſolution of tin; a ſolution of ſulphat of iron reſtores the colour to brown ſtuffs which have been galled; acids reſtore to their former ſplendour yellows which have been rendered duſky or brown by alkalies; blacks produced by logwood become red by acids; alkalies change theſe red ſpots to yellow, and a little of the aſtringent principle makes them again become black. A ſolution of one part of indigo in four parts of ſulphuric acid, diluted with a ſufficient quantity of water, may be employed with ſucceſs to revive the blue colour of cotton or wool which has been changed. Scarlet may be revived by means of cochineal and a ſolution of the muriat of tin, &c.

The choice of re-agents is not a matter of indifference. Vegetable acids are preferable; the ſulphurous acid however may be employed for ſtains occaſioned by fruit: it does not change the blue of ſilk nor colours produced by aſtringents: it does not degrade the yellow of cotton. Ammonia ſucceeds better than fixed alkalies in removing ſpots produced by acids.

It

It is employed in vapour; its action is ſpeedy, and ſeldom alters the colour.

The means of removing greaſy ſpots are well known. This effect is produced by alkalies, fullers' earth, volatile oils diſſolved in alcohol, a heat proper for volatilizing greaſe, &c. Spots occaſioned by ink, ruſt, or iron-mould of any kind, and all thoſe produced by the yellow oxyd of iron, are removed by the oxalic acid: the colour may be reſtored by alkalies, or a ſolution of the muriat of tin. Theſe ſpots may be removed alſo by the oxygenated muriatic acid, when they are on white ſtuffs or paper.

The action of alkalies, and that of perſpired matter, are the ſame: their ſpots may be effaced by acids, or even by a weak ſolution of the muriat of tin. When theſe ſpots ariſe from ſeveral unknown cauſes, in order to deſtroy them recourſe muſt be had to *polychreſt* compoſitions. The following may be conſidered as one of the moſt efficacious. Diſſolve white ſoap in alcohol, and mix this ſolution with the yolks of from four to ſix eggs: add gradually eſſence of turpentine; and incorporate with the whole ſome fullers' earth, in ſuch a manner as to form balls of a ſuitable conſiſtence. Moiſten the ſpot; and, having rubbed it with theſe balls, the ſpot will be removed by waſhing the ſtuff. All ſpots, except iron-mould and ink, may be removed in this manner.

Waſhing deſtroys the luſtre, and leaves a tarniſhed place diſagreeable to the eye; but the luſtre may be reſtored by drawing over the waſhed place, and in the direction of the pile, a bruſh moiſtened in water impregnated with a little gum. You may then apply a ſheet of paper, or a piece of cloth, and a conſiderable weight, under which the cloth muſt be left to dry.

IX. *Deſcrip-*

IX. *Defcription of fome Improvements invented by* Mr. JAMES BURNS, *of Glafgow, applicable to Fire-Grates, Stoves, &c. by which Rooms and Halls of every Defcription may be heated more fpeedily and effectually, and with a fmaller Wafte of Fuel, than by the Methods now in Ufe**.

THE principal merit of Mr. Burns's improvements, and it is not a trivial one, confifts in his contriving to maintain the combuftion of the fuel in *open* ftoves or grates, without employing for that purpofe the air of the room in which they are placed. The heat liberated and thrown out into the apartment is thus prevented from being unneceffarily wafted and hurried up the chimney, as is unavoidably the cafe in the ufual method, where the combuftion is maintained by air from the apartment, the wafte of which is fupplied by exterior cold air that comes pouring in at the bottom of the doors and the fides of the windows, thereby undoing a great part of the effect produced by the fire.

To prevent this wafte, the air that maintains the fire in Mr. Burns's grates, or in others to which his improvements are applied, fhould be brought through a tube from the outfide of the houfe †, or be made to pafs from the outfide of the houfe, between two of the joifts, where the floors and ceilings are found enough to allow this, fo as to be brought to the bottom bars of the grate, without having any communication with the interior air of the room, while at the fame time the grates, and parts connected with them, fhould be fo conftructed, that, when the fire is not wifhed to be fupplied with cold air from the outfide of the houfe, the paffage may be fhut, more or lefs perfectly, by means of a valve, a fmall door, a cock, or any fimilar contrivance. When convenience does not admit of the air-tube being carried to the

* Mr. Burns has lately obtained a patent for thefe improvements.

† The fame principle had been already fuccefsfully applied to *clofe* furnaces: Mr. Howard's, of which we gave an account in our laft Number, furnifhes an example. But its application to common open fires is a new idea, and will probably produce an entire revolution in the conftruction of grates for common ufe.

outfide

outſide of the houſe, if carried to a cellar, larder, ſtair-caſe, or any lumber-room, the ſame end will be gained; with this further advantage, that ſuch cellar, larder, &c. will thereby be well ventilated, and prevented from acquiring or retaining any unhealthy or diſagreeable ſmells.

The form of the grates may be varied indefinitely; but the one we are about to deſcribe anſwers the purpoſe ſo well, and is at the ſame time capable of being made, by the aid of a good architect, ſo highly ornamental to any apartment, that our readers will fully comprehend the facility with which it may be accommodated to circumſtances.

Fig. 1, (ſee Plate V & VI.) repreſents a grate of the improved conſtruction, and Fig. 2 is a ſection of it. The air that maintains the combuſtion is ſupplied through the pipe or tube A, (Fig. 2 and Fig. 4,) or from between the under-ceiling and floor as before-mentioned, from which it paſſes up by the back or ſide of a drawer B, Fig. 2, according as it is moſt convenient to bring it up by the one or the other; the back however is preferred, where convenience admits of it. The front of this drawer, in its place, is ſeen in Fig. 1, at C. The intention of this drawer is to receive the aſhes that fall from the bottom bars of the grate, ſections of which are repreſented at DD, Fig. 2. The aſhes, as they fall from the bars, ſtrike againſt the ſides of the internal cavity E, and then are carried by their own gravity through the narrow part F, and fall into the drawer. This part of the conſtruction may be eaſily underſtood by conceiving it to be an hollow vaſe, as it actually is, with a drawer in its pedeſtal or baſement, and having a grate over it, on which the fire reſts. The valve, door, cock, or other contrivance by which the external air is admitted or excluded, may be either in the neck F of the vaſe, or in the air-tube, or in the drawer B or C, or, which is preferable, in the cavity behind the drawer, the pedeſtal or baſement of the vaſe being made large enough to admit of ſuch cavity. Various conſtructions may be reſorted to; but that repreſented in Fig. 3, (which is a ground-plan of a proper chimney for ſuch a grate, and in which a bird's-eye view of one half of the grate may be ſeen in its place, while the other half repre-

ſents an horizontal ſection of the baſement or pedeſtal, which is ſuppoſed to be open, ſhewing the drawer in its place, with the cavity behind it,) will be found to anſwer every purpoſe. The cavity G is ſeparated from the place in which the drawer is put, by a partition (beſt made of plate or caſt-iron) paſſing from H to H, in which there is an opening I, with a cover K. To the cover K is attached a ſmall bar L, worked by another ſhorter bar M, to which it is attached by any ſimple joint. The bar M is faſtened into an upright pivot, the top of which comes up through the pedeſtal at one of its corners or any other convenient ſpot, and is furniſhed with a button to be laid hold of by the finger and thumb, or with a top fitted to a key, by the turning of which the cover K is made to ſhut or open the aperture I, and conſequently to prevent or facilitate the paſſage of the air from the hole at G, which repreſents the internal mouth of the air-tube, the other end of which is on the outſide of the houſe, or in any cellar or other apartment, as before deſcribed. When the air has paſſed through the aperture I, it finds no difficulty in paſſing on to the bottom of the grate, that back or ſide of the drawer next which the aperture is being made low to allow it to flow in freely.

The grates are recommended to be made of a circular or elliptical form, as being moſt convenient, where the fence or ſcreen, to be immediately deſcribed, is wiſhed to be applied along with Mr. Burns's other improvements. The fence is intended to prevent thoſe dreadful accidents, which too frequently occur, of women's and children's clothes being ſet on fire by ſparks from the grate. When it is wiſhed to be adopted, the inſide of the chimney, where the grate is to ſtand, had beſt be a ſemi-cylinder, or nearly ſo, (as repreſented in Fig. 3,) with a lining or cover *a a a*, beſt made of metal, at ſuch a diſtance from the ſemi-cylindrical wall. NNNN, as to give ſufficient room for allowing the ſafe-guard or fence to be ſlid round into it when the fire is wiſhed to be left open, when freſh fuel is to be added, or when the drawer with the aſhes is to be removed.

The fence is a frame-work of metal, which, when filled up with glaſs or with wire-work, forms a portion of a cylinder

der anſwerable to the curvature of the ſpace between the back of the chimney and the lining above-mentioned, made in one or more pieces, and moving in a circular groove, *bbb*, in or upon the hearth, which ſerves to conduct it into its place behind the grate, when the fire-place is wiſhed to be left open, as before mentioned. The top of the front of the opening of the chimney O, Fig. 4, projects in a circular form, or is furniſhed with an added projection, made of metal, marble, or any other fit material; but in either caſe the projection is furniſhed with a circular groove on its under ſurface, of the ſame radius with the one in, or planted on the hearth, for the purpoſe of receiving the upper part of the frame-work of the fence or ſafeguard, which frame-work may be filled up with glaſs, either plain or bent, and either colourleſs or coloured, or ſtained or painted with figures or deſigns of any kind. By this means complete ſafety is obtained againſt any ſuch accidents as have been alluded to, while at the ſame time the comfort ariſing from the view of a cheerful fire is not prevented by the interpoſition of any opake body; but for nurſeries or the like, where convenience and ſafety is more the object than elegance or luxury, the frame-work may be filled up with wire-work. The fence may be ſeen in its place at P.

Where either the glaſs or the wire-work fence, or both of them, are meant to be applied to ſquare or rectangular chimneys, without the trouble of giving them the ſemi-cylindrical form, the lining to receive the fence or fences may be introduced at the ſides or jambs of ſuch chimneys; or the ſence may be made to riſe by means of pulleys into the wall above the opening, or ſlide ſideways into the walls at the ſides of the opening.

Theſe improvements may be introduced together or ſingly, and may be applied to many of the grates now in common uſe.

Beſides the advantages already pointed out as connected with them, they poſſeſs alſo the following.—Any room or apartment may be heated by their means with a much ſmaller quantity of fuel than by any other method yet in uſe: at the ſame time the advantage of ſeeing the fire is not

 loſt,

loſt, as in cloſe ſtoves; for theſe grates have ſide as well as bottom bars, which allow the radiant heat and light to be thrown out into the room without any impediment: and in fact large rooms, halls, and the like, which by the uſual methods can hardly be warmed, or made at all comfortable in cold weather, may, by means of theſe improvements, be heated as effectually as the ſmalleſt apartment; for, when their full effect is wanted to be produced, it is only neceſſary to keep the fence in its receſs, that even that portion of heat, which would be kept back by the interpoſed glaſs or wire-work, may be thrown out into the room, and perform its office. They are alſo an effectual cure for ſmoky chimneys, which not only cauſe great waſte and deſtruction of good furniture, but many diſeaſes to the inhabitants of houſes plagued with that evil. They cauſe a clean fire-ſide to be eaſily commanded at all times, as hardly any of the duſt or aſhes fall through the ſide bars, almoſt the whole paſſing through the bottom bars down into the drawer; and any fire lighted in ſuch improved grates burns up and becomes lively in a few minutes, without the aid of bellows, and that watchful care which common grates or ſtoves require.

X. *On the Chemical Action of different Metals on each other at the common Temperature of the Atmoſphere. By* C. FABBRONI*.

THAT particular ſenſation, firſt made known by Sultzer, which is felt on the tongue on bringing two metals into mutual contact, and which would have excited none if they had been applied ſeparately to that organ, has been ranked among the galvanic phenomena. C. Fabbroni, however, inſtead of aſcribing theſe effects to an agent almoſt unknown, ſuch as the electric fire, is of opinion that it depends on a chemical operation, perhaps as the ſenſation of taſte itſelf. He has endeavoured to prove the truth of this opinion by a number of obſervations and experiments.

* From *Bulletin des Sciences*, Vol. II. No. 5.

He

He had remarked that feveral metals, fuch as mercury, tin, lead, retained their metallic brilliancy as long as they were pure, while compounds of them became foon tarnifhed and oxydated; that the mere contact of two different metals feemed to haften their oxydation; and that, in this manner, the mixture employed for foldering the plates of copper which cover the obfervatory of Florence, had been fpeedily changed into a white oxyde at the outer edge of its contact with the copper, &c. He thinks that, in this cafe, the metals have a reciprocal action on each other; and that this action, more efficacious and more fenfible when the attraction of aggregation of the metals is deftroyed by fufion, exifts no lefs between folid metals when they touch each other.

If the effects, as rapid as thofe of lightning, occafioned on the tongue by two metals brought into contact, have been by fome afcribed to a peculiar fluid not galvanic, or to the electric fluid, it is becaufe they did not recollect that chemical action is exercifed between two bodies with the utmoft rapidity. The figns of electricity which have been fometimes obferved on feparating two metals in contact, are rather the confequences than the caufe of that action; for it is known that the greater part of chemical operations change the electric equilibrium of bodies, and muft confequently give birth to electrical phenomena. Without totally excluding electricity, therefore, from all galvanic facts, C. Fabbroni thinks that this fluid has fome fhare in the fenfation experienced by the tongue from two metals in contact. This action of metals in contact is proved by the following experiment:—

C. Fabbroni put into two glaffes filled with water, pieces of different metals, one in each glafs. In other glaffes he put two pieces of different metals, but kept them from touching by interpofing a plate of glafs. In a third feries of glaffes he alfo put two pieces of different metals, but in contact. In the metals of the two firft feries he obferved no change, while the moft oxydable metals of the third feries were covered with an oxyde, which confiderably increafed in the courfe of a few days, and the metallic pieces even contracted a ftrong adherence. The quantity of the caloric which difengages itfelf in thefe combuftions is too fmall to be meafured, yet the

light

light which emanates from it may be feen, if the eye itfelf forms a part of the experiment, by holding a piece of filver in the mouth, and applying a bit of tin to the ball of the eye. After thefe two metals are made to communicate, you fee a feeble but diftinct light, which difappears at the end of a few feconds, becaufe the eye becomes accuftomed to this feeble fenfation; but it may be renewed by drawing the metal over the opake cornea, and then over the tranfparent. The author afcribes to a convulfive fenfation that kind of flafh which fome perfons think they perceive * at the moment of the contact of thefe metals, applied one on the tongue, and the other under it.

To make the oxydation of the two metals take place under water, the prefence of air is alfo neceffary. C. Fabbroni thinks that air is ufeful in this circumftance, to add to the water a fufficient quantity of oxygen to be taken up by the metal, as filver is added to gold in order to perform the operation of quartation. If a piece of filver and a piece of tin be put in contact in water, inclofed in a flafk of flint-glafs hermetically fealed, the tin is oxydated; but the oxyde of the lead of the flint-glafs is decompofed, and the glafs becomes black. Philofophers, who afcribe thefe phenomena to electricity, bring, as a proof of their opinion, that they take place when the metals are connected even by means of a pretty long chain. C. Fabbroni has determined the length of this chain at about 18 or 20 feet. Beyond that diftance thefe phenomena are no longer fenfible; while, on the other hand, the electric fluid is communicated to indefinite diftances.

If the phenomena of the experiment of Sultzer belonged really to electricity, they ought to take place with all metals, whatever may be the relation of thefe metals to each other. C. Fabbroni mentions a great number of thefe combinations which produced no effects; and other combinations, of the fame metals, which produced fenfations very diftinct. Thus, if filver be placed on the eye, and gold on the tongue, making them communicate by means of copper, the fenfation is almoft nothing; but it becomes very evident if the iron touches

* The perception is as real as that induced by drawing the metal over the cornea of the eye. Edit.

the

the eye, and the ſilver the tongue, the communication being eſtabliſhed as before with copper *.

In regard to the hydrogen of the decompoſed water, the author thinks that it may alſo be abſorbed by the metal: he even conſiders as an hydro-oxyde of tin, the octaedral cryſtals which he remarked on the ſurfaces of the pieces of tin employed in theſe experiments.—It is clearly ſeen, ſays C. Fabbroni, by the reſults which I obtained from the ſimple contact of two metals in water, that is to ſay, by the oxyde and ſaline cryſtals, that a chemical operation takes place, and that to it we ought to aſcribe the ſenſations experienced on the tongue and by the eye. It appears to me, then, probable, that it is to theſe new compounds, or their elements, that we are indebted for that myſterious ſtimulus which produces the convulſive movements of the animal fibre in a great part at leaſt of the galvanic phenomena.

XI. *On the Art of Hardening Copper.* *By* P. I. Hjelm †.

Copper, in its pure and perfect ſtate, is exceedingly ſoft and malleable: its toughneſs is then ſo great, that it is exceeded only by gold and iron. When copper is hammered a long time cold, and ſtill more when rolled, it is found, as is well known, ſomewhat harder than before, but it does not thereby acquire that ſtrength which deſerves the name of hardening, or which enables it to make ſufficient reſiſtance to ſtrong impreſſions. By being brought to a bright-red heat in the fire, and ſuddenly quenched in water, copper obtains no perceptible addition to its hardneſs; but, on the contrary, becomes more pliable, and conſequently ſofter than before. If the copper is kept a long time in fuſion, or often fuſed in a ſtrong heat, without any covering of flux or charcoal powder, it becomes brittle and unmalleable, and conſequently

* This is no concluſive argument againſt the effect being electrical: it is taking for granted that the laws and properties of the electric fluid have *all* been aſcertained. Edit.

† From *Tranſactions of the Royal Academy of Sciences at Stockholm for* 1797.

harder:

harder: but these properties are soon lost when the copper is melted in contact with carbonaceous matter. If melted copper be poured into water, as is done in the operation of granulating or corning, it does not appear that it acquires any perceptible degree of hardness, or such as can be compared to that which is communicated to steel by the same means.

Considering all these circumstances, which have been confirmed by experience, it could not but excite attention to find, by the most undoubted testimony, that the ancients actually possessed the art of hardening copper, which they employed for instruments of all kinds; such as daggers, swords, bows, shields, javelins, &c. Though ancient authors often mention these weapons, none of them have given us any account of the method of hardening the copper. This deficiency some have attempted to supply by conjectures, which have given rise to a variety of experiments, but not one of them was ever attended with success. As it was imagined that in hardening copper it would be necessary to follow the same process as that employed for steelifying iron, most of their researches have been directed to a similar method; and for that purpose they have not only prescribed, for the hardening of copper, such processes as are employed for hardening iron when it is to be converted into steel, but even the most absurd methods founded upon these processes. These researches, however, instead of answering the intended purpose, only tend to shew the ignorance of those who think they find in the greatest absurdity the most important secrets, and to involve us in still greater darkness.

The art of hardening copper has therefore of late been considered as one of those known to the ancients, which were afterwards lost; and it would no doubt have still remained in that state, had not several of these monuments of our ancestors been brought to light by some fortunate accidents, and the respect for their great antiquity been overcome by a desire for becoming acquainted with their composition. When this method, the only certain and decisive one, was once chosen, it was no great difficulty to discover that the whole art did not depend on any process like that employed in regard to iron, but on the addition of a certain quantity of

 some

ſome other metal melted with the copper, by which it was rendered harder than before.

The abbé Mongez firſt wrote a treatiſe* on the metallic compoſition of the ancient bells, which he tranſmitted to the Academy of Inſcriptions. He found that this compoſition was in general nearly the ſame as that ſtill uſed for bell-metal, that is, copper and tin. M. Mongez afterwards tranſmitted to M. Dizé the point of an ancient copper dagger, the appearance of which on the fracture ſufficiently ſhewed that it had been fuſed and caſt. When diſſolved in pure nitrous acid, there remained a white powder, found to contain tin, and that which had been diſſolved was merely copper. It evidently appeared, therefore, that the ſuppoſed art of the ancients for hardening copper, was nothing elſe than fuſing it with a certain quantity of tin. This was ſtill farther confirmed when M. Dizé found the ſame component parts in ſome Greek, Roman, and Gallic coins, which he obtained on the ſame occaſion for the purpoſe of examination. The tin in them formed about twenty-four parts in a hundred of the mixture.

In ſupport of this opinion, reſpecting the art of hardening copper among the ancients, I have the honour of laying before the Academy ſome experiments lately made on this ſubject.—Laſt ſummer, Profeſſor A. I. Retzius, of Lund, tranſmitted to me a part of the blade of a two-edged dagger, which, together with ſome ſtone-cutters' chiſſels, were found in a hill of earth on the low lands of Scandinavia, where whole ſwords of ancient workmanſhip have often been found. This fragment, on the outſide, had rather a yellowiſh appearance like braſs, than the red colour of copper; the edge was exceedingly thick, and roundiſh; the fracture ſeemed alſo granulated, which evidently ſhewed that the work had been caſt. When tried by the file, it was not quite ſo hard as the common bell-metal, but ſomewhat harder than common gun-metal. The newly-filed ſurface had a reddiſh-yellow appearance, but ſoon became yellower. When melted by the blow-pipe, this compound metal exhibited no traces of zinc; but it might be readily ſeen, by its appearance, that the greater part of the mixture was copper. The filings were

not in the leaſt attracted by the magnet, which clearly proved that there was no iron in the compoſition. It exhibited as little traces of any other metals, as far as could be judged from the aſcending vapour. In order to aſcertain with what metal the copper had been mixed, twenty-five aſſayer's pounds of the pureſt filings of the above fragment were collected; pure nitrous acid was heated in a glaſs retort, and diluted with diſtilled water; and a few of the filings were thrown into it: when the firſt quantity was diſſolved, a few more were added, and this was continued till the whole quantity was put in. The ſolution was made to boil for a quarter of an hour, and then diluted with a little more diſtilled water, in order that a white powder, which remained undiſſolved, and which I ſuſpected to be calx of tin, might more readily fall to the bottom. After this was done, the bright blue-coloured ſolution, which was found to contain nothing but copper, was carefully poured out, and diſtilled water was poured over the white powder, which was waſhed ſeveral times in the ſame manner, after it had each time remained at reſt long enough to allow it to ſettle, when it was thrown upon filtering paper to be farther waſhed. This white powder, when dry, weighed $5\frac{3}{8}$ aſſayer's pounds, which gave $21\frac{1}{2}$ pounds of tin calx in the whole hundred. Now, tin calx loſes, by being revived, $\frac{1}{4}$, or 25 *per cent.* of its weight; and therefore this $21\frac{1}{2}$ pounds of tin calx muſt have given $16\frac{1}{8}$ pounds of metallic tin in the hundred, which, in an experiment made for the purpoſe, was found to be nearly the caſe, and the regulus obtained was found to be pure. The metallic compound of the before-mentioned dagger blade was made therefore of $83\frac{7}{8}$ copper, and $16\frac{1}{8}$ tin; or, to reckon without fractions, of 84 parts of copper, and 16 parts of tin.

By way of experiment, a compound of this kind, conſiſting of pure copper and pure tin, was fuſed, and a penknife blade was made of it, which was poliſhed and ground in the uſual manner. This blade had all the properties and the external appearance of the fragment of the dagger. Where the edge was thin, it was eaſily turned: it was therefore thought that perhaps, in the dagger before mentioned, the proportion of tin was greater. To aſcertain this point, another compoſition

was

was made, of 20 parts of tin and 80 of copper, and a knife blade was made of it, as before. This was much whiter and harder, but alfo brittler in the fame proportion, and therefore broke by careleffnefs in the polifhing. The edge, however, was fo fharp that it could be ufed for making pens; but it did not ftand long, as notches were formed in it by each cut.

When the tin makes twenty-five hundred parts in the mixture, it becomes rather white than red, but exceedingly brittle. If the addition of tin be increafed to thirty in the hundred, or more, both thefe properties are increafed in the fame proportion, and the compofition becomes fit for fpecula. In gun-metal tin makes nine parts, or more, in the hundred: in bronze, 84 parts of copper are mixed with about 16 parts of tin, but a confiderable portion of zinc or brafs is fometimes fubftituted for the latter. Bell-metal contains in general 76 parts of copper, 19 of tin, and 5 of brafs, or thereabouts.

What has been here faid may perhaps be fufficient to confirm the opinion refpecting the art employed by the ancients to harden copper, and may furnifh fome hints for the employment of fuch compounds in common life. Befides, we are hereby enabled to appreciate the different opinions entertained on this fubject. M. Monnet imagined that the copper in ancient times was mixed with arfenic, which rendered it hard. No real objection can be made to this being poffible; but as long as no ancient implements made of this mixture are found, the above affertion may at any rate be doubted, without mentioning other circumftances which feem directly to oppofe it.

M. Dizé * mentions the addition of iron to copper, as the means of rendering the latter harder; and endeavours to prove that Geoffroy the younger, who firft drew this conclufion, (from an experiment he made, where copper, mixed with fixteen parts of iron in the hundred, was found to be equally hard, to have the fame grain on the fracture, and to be as fit for making cutting inftruments as the hardened copper of the ancients,) was too precipitate in forming his

* *Journal de Phyfique* 1790, April.

opinion.

opinion. The queſtion, however, aſſumes a different appearance, when we take into conſideration the experiment as related by Count Caylus, in his work on the Egyptian, Etruſcan, Greek and Roman antiquities *: for it there appears that M. Geoffroy undertook the above-mentioned experiment by the deſire of Count Caylus, who deſcribes various kinds of arrows and javelins of ancient workmanſhip, which, though they had the appearance of copper, were mixed with iron; becauſe filings of them were attracted by the magnet; becauſe the fracture had a different appearance from that of other inſtruments made of the hardened copper of the ancients mixed with tin, and was at the ſame time leſs fuſible. M. Geoffroy thinks it rather remarkable that mankind ſhould ſo early have fallen on the method of uniting copper with iron in an uniform mixture, which even at preſent is conſidered as a difficult proceſs. It is well known that the moſt common copper ore conſiſts of copper and iron mineraliſed with ſulphur, and which is called pyrites of copper; not that the copper makes the greater part of it, but becauſe the copper is of the greateſt value, though the iron ſeems moſt generally to conſtitute the principal component parts. When this ore is ſmelted, the firſt copper obtained, or the ſo called black or raw copper, is neceſſarily rendered impure by a greater or leſs quantity of iron, according as more or leſs care has been employed to ſeparate it during the operation. It is therefore in our power, it is ſaid, not only to obtain copper combined in this manner with as much iron as may be neceſſary, but alſo to caſt all kinds of inſtruments of it, and afterwards to hammer them cold, or to expoſe them to the ſame proceſs of hardening as if they were of pure ſteel. Some aſſert that this method has been attended with complete ſucceſs. Count Caylus tried alſo to harden pure copper by melting; but, inſtead of becoming hard, it was found ſofter and more malleable, which agrees with what has been ſaid in the beginning of this paper.

Without in the leaſt leſſening the credibility of this aſſertion, which ſeems to have great probability in its favour, we

* Recueil d'Antiquités Egyptiennes, Etruſques, Grecques et Romains. Vol. I. p. 238—251.

may

may at leaſt expreſs a wiſh that ſome perſon would make, of copper mixed in this manner with iron, different inſtruments, and then endeavour to ſhew real antique works made of the ſame mixture, in order that they may be compared. Until this be accompliſhed, it will be beſt to adhere to that proceſs which ſeems to have been chiefly followed, and of which indubitable proofs are ſtill in exiſtence.

The hiſtory of the antiquity and uſe of metals in the period to which this queſtion properly alludes, is involved in ſo great darkneſs, that nothing deciſive on the preſent ſubject can be derived from it. This much, however, is certain, that the works of ancient authors ſtill extant ſpeak of gold, ſilver, copper, iron, tin, and lead, as known at the ſame time, and employed for various purpoſes. In regard to copper in particular, it is found more abundantly in a native ſtate than any other metal, and requires nothing farther than ſmelting to be immediately uſed. In proceſs of time mankind would become acquainted with the art of extracting the metal from other ores of copper; not, however, without greater labour, and therefore at firſt none but rich ore could be uſed; and we may with juſtice conclude, that copper was one of the firſt metals worked.

Tin is not found in a native ſtate, but its ore is abundant in certain places, and is eaſily revived, or brought to the metallic ſtate, eſpecially when people are acquainted with the proceſs of ſmelting other metals. The antiquity of tin, therefore, is as well eſtabliſhed as that of the other metals among which it is named. In the time of the Tyrians a conſiderable trade was carried on in this metal, which they brought from the Caſſiterides iſlands, beyond the pillars of Hercules, under which name England is no doubt meant.

Whether the art of mixing theſe two metals together by fuſion was firſt found out by accident, or by experiments made for the purpoſe, it is not to be doubted that caſt works of ſuch a compoſition are mentioned at the ſame period with the ſimple metals. Beſides, the works in bronze of the ancients are a ſufficient proof of their ſkill in combining metals, of their art of modelling after Nature, and of their readineſs

readineſs in caſting. All their inſtruments and edged tools of this kind hitherto found have been caſt, and not hammered; and of this kind, without doubt, were the inſtruments mentioned by Profeſſor Pallas as having been found near the Tſchudi mines. All of them, except a few, conſiſted of a compoſition of copper and tin.

The art of preparing and ſeparating iron may have been diſcovered as early as that of preparing other metals. As this art, however, requires a greater degree of dexterity, it was doubtleſs leſs common at firſt, and muſt have been diffuſed more ſlowly. This muſt have been the caſe much more with the art of preparing ſteel, which naturally would be much later than that of preparing iron, and melting tin and copper. The method of preparing ſteel may have been generally known in one country before it was communicated to another, where copper, hardened in the above manner, may have been uſed in its ſtead. The Japaneſe ſtill uſe mirrors of white copper, which conſiſts of a mixture of that metal and tin. The metal of the Chineſe gongs conſiſts of copper, mixed with 18 parts in a hundred of tin, and probably a little nickel; and theſe inſtruments are at the ſame time ſubjected to ſtrong hammering when cold. The Chineſe form their ill-ſhaped razors of iron (not hammered) filed to an edge, and which cut ſo badly, that, after every ſtroke on the beard, they muſt be drawn over a file. Many tribes who have been diſcovered in modern times, and who are unacquainted with the uſe of metals, employ hard kinds of ſtones for making knives, hammers, arrow-heads, axes, &c. Induſtry always ſupplies itſelf with its neceſſaries, and employs for that purpoſe ſuch materials as can be obtained, without taking into account the degree of labour which the preparation of them may require.

XII. *Experi-*

XII. *Experiments on the Nature and Properties of the Pietra fongaja, Lapis fungifer**. *By* P. A. Gadd †.

STONES in general are in Italy called *Pietra*; a word which fometimes is ufed to denote the harder fort of ftones, as well as ftones of a certain genus; for example, *Pietra bigia, obfidiana, nephritica*, &c.; and this is the cafe with the fo called *Pietra fongaja*. I. I. Ferber may be confidered as the firft perfon who gave a defcription of it in his letters from Italy. He has remarked alfo that a kind of it is in common ufe in the houfes of Naples and Rome; and that he faw another kind in the poffeffion of M. Fabbroni, at Florence: the firft kind, which was found in the chalk-hills near Naples, confifted of white calcareous ftalactites, and a number of fmall roots of vegetables; the latter was a hardened turf, dug up in the neighbourhood of fome volcanic mountains.

A few years ago, M. Charles Spärre, chancellor of the academy, having been fo kind as to tranfmit to me a piece of *pietra fongaja*, which he had brought with him from Italy. I analyfed it, and found the refult as follows:—It burns in an open fire, and emits the fmell of putrid vegetables. When burnt in a ftrong fire, the greater part of it becomes dark-grey afhes. A hundred parts of this hardened turf, loft about fifteen parts in weight. When fufed in a ftronger heat, it is converted into a black opaque flag, which, however, is difficult to be fufed. When a fmall part only of this earth is fufed with borax, the glafs acquires a dark-green colour. If a little water be poured over the calcined earth, the water exhibits traces of diffolved pot-afh, but the earth does not appear to have thereby fenfibly decreafed in weight. One hundred parts of the earth, previoufly calcined in a crucible, being analyfed, were found to contain about 45 or 46 of filiceous earth, 23 argil, 7 calcareous earth, 20 calx of iron, together with traces of magnefian earth and pot-afh.

When the *pietra fongaja* is kept in a cellar, and moiftened

* Mufhroom-ftone, or mufhroom-bearing ftone.

† From *Tranfac. Royal Acad. Stockholm for* 1797.

with

with water, it produces a great many eatable muſhrooms, which in Italy are ſerved up at the tables of the great as delicacies. It needs excite no wonder that muſhrooms ſhould grow on the *pietra fongaja*, ſince a multitude of fruitful muſhroom ſeeds are intermixed in this ſoft ſtalactites, as well as with the hardened turf found near volcanoes.

For the information of thoſe who may be deſirous of making muſhrooms continually grow up from the *pietra fongaja*, and of increaſing the quantity, it may be neceſſary to remark, that this effect will be produced, if, according to the experiment of M. Gleditſch, the muſhroom-ſtones kept in cellars be moiſtened with water in which muſhrooms have been waſhed *.

XIII. *On the Volcanic Iſland thrown up in the Neighbourhood of Iceland. By Captain* VON LÖWENÖRN, *of the Daniſh Navy* †.

IN the ſpring of the year 1783, a volcanic iſland, thrown up in the neighbourhood of Iceland, excited no little attention. According to the account of the ſea-captain, who firſt ſaw it, exactly at the time when it firſt aroſe, ſmoke and flame ſeemed to riſe from the ſea, but no land or iſland was to be ſeen. It needs excite no wonder, therefore, that the obſerver was thrown into the greateſt conſternation, as he ſays himſelf, when he beheld the ſea on fire! He and the whole crew therefore concluded that the end of the world had arrived, and they all began to prepare themſelves for the awful moment: but, as they heard no trumpet, and as the ſun ſhone in the firmament with his uſual brightneſs, after conſidering what the phenomenon might be, they at laſt concluded that Iceland had been ſwallowed up by an earthquake; that this was a remainder of it; and that the flames aroſe from Hecla, the well-known volcano of that iſland. Full of this idea, they were juſt on the point of re-

* Does not this furniſh a hint to thoſe who rear muſhrooms in gardens on beds of horſe-dung? EDIT.

† From *Geographiſche Ephemeriden*, 1799.

turning, in order to convey intelligence of this event to Denmark; but very luckily they soon after discovered the coast of Iceland.

The place where this volcanic eruption was seen, lies only $7\frac{3}{4}$ nautical miles, fifteen to a degree, from the south-west extremity of Iceland. Hitherto they had seen no land, but recognising Iceland, the ship reached the place of destination, and completed her voyage. Other ships, which arrived later, saw a small island from which the eruption had arisen; but it always exhibited, as might naturally be expected, a different appearance. The same year smoke and flames were seen to arise from the nearest part of the opposite coast of Iceland.

As there have been many instances of such eruptions from the sea producing islands, this event attracted the notice of government, and the year following orders were given to the ships bound to Iceland to examine the new island; but it had entirely disappeared, so that no traces of it were to be found. Towards the conclusion of the year, however, an unfortunate accident happened, which, without doubt, was occasioned by sunken rocks forming a part of the island which had disappeared.

A Danish ship of war of 64 guns, called the *Infödsretten*, was expected from the East Indies, and intelligence had already been received that she had sailed from the Cape of Good Hope; but after that period no farther account was heard of her till the year 1785, when some vessels returning from Iceland reported, that some fragments of this ship, together with the long-boat, had been driven ashore on the coast of that island. According to every account, and by comparing the different circumstances, it appears to me certain that the above ship was wrecked on these rocks, then no longer visible above the surface of the sea. It is impossible that such a large boat could have been conveyed from a ship without the hands of men, unless the ship had been dashed to pieces. This boat was not only driven to land entire, and in good condition, though without any person in it, but there was found in it a box filled with wax candles. At the distance of about a quarter of a mile from the boat there were

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found various pieces of the ſame ſhip, which could be eaſily known by ſome diſtinguiſhing marks. Theſe parts, of different forms and ſizes, would not have been thrown on ſhore ſo near each other if the misfortune had happened at a greater diſtance; the billows, currents, &c. would certainly have driven them on ſhore at places more remote from each other. Beſides, theſe fragments were driven on ſhore by a wind which blew in a direction from theſe rocks, and nothing elſe of this misfortune had been perceived on the coaſt.

From all theſe circumſtances I conclude that this veſſel had experienced a very bad voyage home from the Cape of Good Hope, for that year eaſterly winds were exceedingly prevalent in the northern ſeas. A great many ſhips, and particularly men of war, preferred going round Great Britain to the paſſage through the Channel. It is probable that the ſhip in queſtion may have been in want of ſome neceſſary, ſuch perhaps as freſh water. The captain, beſides, was well acquainted in Iceland, where I myſelf was with him, ſome years ago, as lieutenant on board a ſhip which he then commanded, and on this account he probably intended to run into ſome of its harbours, but unfortunately ſtruck on the ſunken rocks, the remains of the volcanic iſland. In this diſtreſſed ſituation the crew, no doubt, had recourſe to the only probable means left for ſaving their lives by hoiſting out their long-boat, and while employed in this labour the ſhip, it is likely, went to pieces, and the people were loſt, as none of them were ever ſeen or heard of.

During my expedition to Iceland in the year 1786, I made it my buſineſs to make ſome reſearches in regard to this volcanic iſland, though at that time no ſuſpicion was entertained that the above ſhip had been wrecked in this place; for this conjecture was only a conſequence drawn from my reſearches.

When I arrived in Iceland, where, on account of the buſineſs entruſted to my charge, as well as on account of the obſervations which I was obliged to make for the improvement of charts, I found it neceſſary to remain ſome time with my ſhip in Holmens-hafen, and had at my diſpoſal a ſmall veſſel which was lying there, I ordered Lieutenant,

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now Captain Grove, to cruiſe about with this ſmall veſſel in the neighbourhood of the place where the volcanic iſland had been ſeen. He remained there ſome days, and though he often ſounded with a line of more than a hundred fathoms, found no bottom, ſo that he loſt all hope of making any diſcovery; but, juſt when he was about to return, he obſerved, contrary to all expectation, that the waves broke over ſome rocks lying exactly level with the ſurface of the water. As he now entertained no doubt that he had found what he had been ſent in queſt of, he took the bearings and diſtances from the neareſt part of the coaſt of Iceland, and tranſmitted to me an account of his obſervations.

When the buſineſs of the expedition was ended, and I was about to return at the end of the ſummer, I reſolved to viſit this intereſting point myſelf, and to aſcertain its real poſition by actual obſervation. I took my departure, therefore, from ſome ſmall iſlands, or rocks, which lie before Cape Reikianös, the ſouth-weſt extremity of Iceland, and the outermoſt of which is called the Grenadier's Cap, diſtant $3\frac{1}{4}$ miles ſouth-weſt from the Cape. As the weather was exceedingly favourable, I was ſo fortunate as to obtain its latitude by the meridian altitude of the ſun, and its longitude by a timekeeper. Though the timekeepers which I carried with me were not of the beſt kind, as I had quitted the ſame day one of the ports of Iceland, where I obſerved their rate of going, their relative errors could not be of great importance. I determined, therefore, the poſition of the rock called the Grenadier's Cap at 63° 43′ 40″ north latitude, and 25° 35′ 40″ weſt longitude from the meridian of Paris. This agreed pretty nearly with the obſervations of Verdun de la Creune, Borda, and Pingré *; eſpecially as I have good reaſon to believe, that, from a want of ſufficient knowledge of the coaſt of this country, they placed Cape Reikianös three minutes too far north, as they make the latitude to be 63° 55′. As I found alſo, by the moſt accurate obſervations that could be made at ſea, that theſe dangerous rocks lie 47° in a direction ſouth-weſt from the true meridian, and juſt four

* *Voyage fait par ordre du Roi en* 1771, 1772.

miles from the before-mentioned Grenadier's Cap, the poſitions of theſe rocks will be 63° 32′ 45″ north latitude, and 20° 2′ 50″ weſt longitude from the meridian of Paris.

As I now proceeded to get a ſight of theſe rocks, Captain Grove, who was on board my ſhip, concluded, from his former obſervations, that we could be at no great diſtance from them, having now quite loſt ſight of the Icelandic coaſt; and the before-mentioned rocks, which lie to the ſouth-weſt from Iceland, though the weather was clear, being now ſcarcely diſcernible. My companion, therefore, aſked whether it was prudent to advance ſo ſtraight upon it. While we were talking on this ſubject the people called out, and immediately every eye was directed to the ſpot, where we ſaw before us the waves breaking over a rock. We immediately put about ſhip, and heaved the lead, which was in readineſs, and found the depth twenty-ſix, and ſoon after forty fathoms, but a little farther no ground was to be found with a line of a hundred fathoms. Some tallow had been put into the bottom of the lead, as uſual, to enable us to determine the nature of the bottom by the ſubſtances which adhered to it. By theſe means we obtained ſmall fragments of ſtone which were entirely lava, or of a volcanic nature. The rock is not large, and the water around it is exceedingly deep. Its height is exactly equal to that of the ſurface of the ſea, or rather a little lower; and for that reaſon it cannot be ſeen till one approaches very near to it, or when the waves break over it.

The origin of the volcanic iſland, which was ſeen in this place in the year 1783, I explain in the following manner:—The rock which now remains formed the crater, which at that period threw up flames and ſmoke. The large quantity of lava which iſſued from it, being accumulated on the bottom of the ſea around the crater, may at length have been raiſed above the ſurface of the ſea, and even to a conſiderable height. But as this volcano lies in a part of the ocean where prodigious billows prevail, and roll over each other throughout a wide extent of ſea, it is probable that ſuch a ſtructure would ſoon be deſtroyed by their violence, eſpecially as there is a great depth of water around it, in which it might eaſily

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be overturned. It is known alfo that the fame year a confiderable quantity of pumice-ftone and volcanic fubftances of the like kind, the fpecific gravity of which was lighter than that of water, was caft on fhore in Iceland, and found floating on the fea by mariners.

Had the eruption taken place in a calmer fea, and the depth around it been lefs abrupt, the thrown up mafs would have confolidated itfelf by its own weight, and would have in time become an ifland; of which we have had inftances in the Archipelago, in the Eaft Indies, and different parts of the ocean. Had it taken place on the continent, or in an ifland, it would have formed a mountain. It is not neceffary that a volcano fhould always arife from a mountain: volcanoes have been feen to burft forth in plains; but the invariable confequence is, that the volcanic matter, by being accumulated, and, as it were, piled up, forms a mountain. Now, as the violence of the waves may have eafily wafhed away the loofe matter accumulated round the crater, there is no abfurdity in fuppofing, that, as the billows rolled over the mouth of the crater, the fire was at length overcome by the water, and the volcano extinguifhed.

The crater, confifting of rock, has remained. It is well afcertained that a rock exifted in this place before the eruption; and it is confirmed, by late obfervations, that it exifts ftill. An obfcure notion prevailed among the feamen who frequented Iceland, that there was a blind rock* in this neighbourhood called *Fugle-Skiær* (Bird's rock). This name I have retained in my charts, though the exiftence of it is denied by many feamen, becaufe they paffed without feeing it. But, under fuch circumftances, the teftimony of one who has feen it is of more weight than a hundred who deny its exiftence becaufe they did not fee it. This confirms me in the opinion that the crater had exifted long before in the fame ftate.

To conclude, it may not be fuperfluous to remark, in order to ftrengthen this opinion, that, nearly in the fame direction from the fouth-weft extremity of Iceland, as already mentioned, there are five fmall iflands or rocks, the outer-

* Rocks lying under the water, and which are therefore more dangerous, are by feamen called *blind rocks*.

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moſt of which lies at the diſtance of $3\frac{1}{2}$ miles from Cape Reikianös. Between theſe the water is deep; ſhips which go to, or come from, the weſt ſide of Iceland, commonly paſs through them, when they firſt get ſight of the land and rocks. By the Daniſh ſeamen they are called *Fugle-Skiär*, becauſe they are frequented by a great number of ſea-fowl; but by the inhabitants they are called *Eld Eyarne*, (fire iſlands.) May not this afford reaſon to conjecture that in former times they had volcanic eruptions? and the volcano which appeared in the year 1783 may probably have exiſted long before.

XIV. *Experiments on ſome peculiar Matters drawn from Animal Subſtances treated with the Nitric Acid.* By C. Welter *.

The author having treated ſilk with the nitric acid, to obtain from it the oxalic acid, was ſurpriſed to find that at the end of his proceſs he obtained a ſilky-looking ſalt of a golden-yellow colour, and which, on the approach of a piece of red-hot coal, exhibited all the effects of gunpowder. As he made the experiment only once, he thought it of importance to give a particular account of the proceſs, in order that it might be repeated.

On one part of ſilk he poured ſix parts of nitric acid of the ſhops, adding a little concentrated nitric acid. After it had reſted two days, he diſtilled this mixture. He then poured what had paſſed into the receiver, on what remained in the retort, and filtered the whole. The oxalic acid cryſtalliſing on the filtre, he put the whole again into the retort, and added a pretty large quantity of water, which had ſerved to waſh the filtre. He diſtilled off a part of the water; but as the reſiduum did not cryſtalliſe, returned, by elevating the receiver, what had paſſed over; and, after repeating this operation ſeveral times, obtained for reſiduum an acid liquor of the weight of the ſilk employed, and which contained ſmall granulated cryſtals.

This liquor ſhewed no traces of the oxalic acid. It was yellowiſh, and communicated that colour to the fingers and

* *Bulletin des Sciences*, Vol. II. No. 1.

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to ſilk. The tint was not in the leaſt weakened by waſhing. C. Welter ſaturated this liquor with lime; and having concentrated it, he added alcohol, which took up a matter of a gummy appearance. The alcohol, diluted with water, being evaporated, there remained a yellow ſubſtance mixed with ſolutions of the nitrat and muriat of lime. Theſe ſalts were decompoſed by carbonat of pot-aſh, and the liquor, ſeparated from the carbonat of lime, was ſubjected to evaporation. It gave golden-coloured cryſtals, which had the fineneſs of ſilk; and detonated like gunpowder, producing a black ſmoke. Theſe cryſtals are ſoluble in water and alcohol, and cryſtalliſe on cooling. They are deprived of their colour by the oxygenated muriatic acid. The ſulphuric acid diſengages from them the odour of the nitric acid. The muriatic acid precipitates, from a ſolution of them, ſmall micaceous, whitiſh, volatile cryſtals, which in the fire exhale a bitter and inflammable ſmoke.

This golden-yellow coloured detonating and cryſtalliſable ſubſtance is by the author called *amer* (bitter); its cryſtals appear to be octaedral. As animal ſubſtances become yellow by the contact of the nitric acid, C. Welter endeavoured to extract *amer* from raw beef; but he found it combined with another ſubſtance, which, like it, could not be altered by the nitric acid. This combination, ſoluble in the concentrated nitric acid, is ſeparated from it by water, under the form of a yellow powder, which does not loſe its colour by expoſure to the air, and which might perhaps be uſeful in painting.

What made C. Welter preſume that this powder is compoſed of *amer* and another ſubſtance, is, that he obtained the latter by treating ſponge with the nitric acid. It is colourleſs, ſoluble in concentrated nitric acid, and ſuffers itſelf to be precipitated by water like the preceding powder.

What has been here ſaid ſeems to ſhew that animal matters treated with the nitric acid give as reſiduum two ſubſtances unalterable by that acid, and which are found either in the ſtate of combination, or ſeparate. It appears that ſilk gives pure *amer*, ſponge gives the ſecond ſubſtance pure, and beef a combination of both. The *amer* is yellow, and ſoluble in water: the combination of both is as inſoluble in water as the ſubſtance obtained from ſponge, but coloured.

XV. *Reflec-*

XV. *Reflections on the Quality of Earthen-Ware, and the Results of the Analysis of some Earths and common Kinds of Earthen-Ware.* *By* C. VAUQUELIN*.

FOUR things may occasion the difference in the qualities of earthen-ware: 1st, the nature or composition of the matter: 2d, the mode of preparation; 3d, the dimensions given to the vessels; 4th, the baking to which they are subjected. By composition of the matter, the author understands the nature and proportions of the elements of which it is formed. These elements, in the greater part of earthen-ware, either valuable or common, are silex, argil, lime, and sometimes a little oxyd of iron. Hence it is evident that it is not so much by the diversity of the elements that good earthen-ware differs from bad, as by the proportion in which they are united. Silex or quartz makes always two-thirds at least of earthen-ware; argil or pure clay from a fifth to a third; lime from 5 to 20 parts in the hundred; and iron from 0 to 12 or 15 parts in the hundred. Silex gives hardness, infusibility, and unalterability; argil makes the paste pliable, and renders it fit to be kneaded, moulded, and turned at pleasure. It possesses at the same time the property of being partially fused by the heat which unites its parts with those of the silex; but it must not be too abundant, as it would render the earthen-ware too fusible and too brittle to be used over the fire.

Hitherto it has not been proved by experience that lime is necessary in the composition of pottery: and if traces of it are constantly found in that substance, it is because it is always mixed with the other earths, from which the washings and other manipulations have not been able to separate it. When this earth, however, does not exceed five or six parts in a hundred, it appears that it is not hurtful to the quality of the pottery; but if more abundant, it renders it too fusible.

The oxyd of iron, besides the inconvenience of communicating a red or brown colour, according to the degree of

* *Bulletin des Sciences*, Vol. II. No. 2.

 baking,

baking, to the veffels in which it forms a part, has the property of rendering them fufible, and even in a greater degree than lime.

As fome kinds of pottery are deftined to melt very penetrating fubftances, fuch as falts, metallic oxyds, glafs, &c. they require a fine kind of pafte, which is obtained only by reducing the earths employed to very minute particles. Others deftined for melting metals, and fubftances not very penetrating, and which muft be able to fupport, without breaking, a fudden tranfition from great heat to great cold, require for their fabrication a mixture of calcined argil with raw argil. By thefe means you obtain pottery, the coarfe pafte of which refembles *breche*, or fmall-grained pudding-ftone, and which can endure fudden changes of temperature.

The baking of pottery is alfo an object of great importance. The heat muft be capable of expelling humidity, and agglutinating the parts which enter into the compofition of the pafte, but not ftrong enough to produce fufion; which, if too far advanced, gives to pottery a homogeneoufnefs that renders it brittle. The fame effect takes place in regard to the fine pottery, becaufe the very minute divifion given to the earths reduces them nearly to the fame ftate as if this matter had been fufed. This is the reafon why porcelain ftrongly baked is more or lefs brittle, and cannot eafily endure alternations of temperature. Hence coarfe porcelain, in the compofition of which a certain quantity of calcined argil is employed, porcelain retorts, crucibles, tubes, and common pottery, the pafte of which is coarfe, are much lefs brittle than difhes and faucers formed of the fame fubftance, ground with more labour.

The general and refpective dimenfions of the different parts of veffels of earthen-ware have alfo confiderable influence on their capability to ftand the fire.

In fome cafes the glazing or covering, efpecially when too thick, and of a nature different from the body of the pottery, alfo renders them liable to break. Thus, in making fome kinds of pottery, it is always effential, 1ft, to follow the beft proportion in the principles; 2d, to give to the particles of

the paste, by grinding, a minuteness suited to the purpose for which it is intended, and to all the parts the same dimensions as far as possible; 3d, to carry the baking to the highest degree that the matter can bear without being fused; 4th, to apply the glazing in thin layers, the fusibility of which ought to approach as near as possible to that of the matter, in order that it may be more intimately united.

C. Vauquelin, being persuaded that the quality of good pottery depends chiefly on using proper proportions of the earthy matters, thought it might be of importance, to those engaged in this branch of manufacture, to make known the analysis of different natural clays employed for this purpose, and of pottery produced by some of them, in order that, when a new earth is discovered, it may be known by a simple analysis whether it will be proper for the same object, and to what kind of pottery already known it bears the greatest resemblance.

	Hessian Crucibles.	Argil of Dreux.	Porcelain Capsules.	Wedgewood's Pyrometers.
Silex	69	43·5	61	64·2
Argil	21·5	33·2	28	25
Lime	1	3·5	6	6
Oxyd of iron	8	1	0·5	0·2
Water	-	18	-	6·2

Raw kaolin 100 parts.—Silex 74, argil 16·5, lime 2, water 7. A hundred parts of this earth gave eight of alum, after being treated with the sulphuric acid.

Washed kaolin 100 parts.—Silex 55, argil 27, lime 2, iron 0·5, water 14. This kaolin, treated with the sulphuric acid, gave about 45 or 50 per cent. of alum.

Petuntzé.—Silex 74, argil 14·5, lime 5·5, loss 6. A hundred parts of this substance, treated with the sulphuric acid, gave seven or eight parts of alum. But this quantity does not equal the loss sustained.

Porcelain of retorts.—Silex 64, argil 28·8, lime 4·55, iron 0·50, loss 2·77. Treated with the sulphuric acid, this porcelain gave no alum.

XVI. *Eleventh*

XVI. *Eleventh Communication from Dr.* THORNTON, *Physician to the Mary-le-bone General Dispensary,* &c. &c. &c. *relative to Pneumatic Medicine.*

A REMARKABLE CURE OF AN ULCER OF THE LEG.

MR. RODERICK M'KENNON, aged 67, went in the year 1758 as Affiftant Apothecary to St. George's Hofpital, where he had his wafhing, board and lodging found him, with a fuitable falary. In June 1795, whilft in this employ, he went to fee Dr. M'Nab, who then refided in Great Suffolk-ftreet; and as he was at the door, a bitch in the houfe, who had puppies, furioufly flew at him, and feized him near the calf of the leg, making a deep lacerated wound. The wound foon after became dreadfully inflamed, poultices were applied, and it was near a fortnight before he made his cafe known to the furgeons of the hofpital. He was now confined to his room, and thefe moft experienced and eminent practitioners continued their humane attentions to him above a twelve-month, trying a variety of different applications, until, finding his cafe baffle all their endeavours, he was difmiffed his employ, and left the hofpital as incurable. Added to this dreadful and unforefeen affliction, he had an afthma, which had exifted on him above ten years, and was obliged frequently to fit up the greateft part of the night with the windows wide open to procure breath. He was now in the vale of years, and with a gloomy profpect before him; for no falary was allowed this almoft fuperannuated fervant of a public charity, to which he had been attached above thirty years; and he had a wife and daughter to provide for. After quitting the hofpital, Mr. Carpue, a furgeon no lefs diftinguifhed for zeal than abilities, for fome months attended him; but finding all his endeavours ineffectual, he reluctantly took his leave of him as incurable. Such was the deplorable ftate of this unfortunate fufferer, when Mr. Carpue recommended him for the trial of the oxygen air, ufing thefe very expreffions: "Poor Roderick has been under Mr. Home's care," (an eminent furgeon, brother-in-law to John Hunter,) "in

St. George's Hoſpital, which he left as incurable, and ſince under my care for ſeveral months; and ſo bad is his caſe, that I am ſure if you can cure him you can cure the devil." Being no ſurgeon, I could have no wiſh to accept of ſuch a caſe but for the cauſe of humanity and the ſake of ſcience; and I feel extreme delight in ſaying, that poor Roderick is now perfectly cured, the ulcer is healed, his aſthma gone, and, in order that the philoſophic world may ſee fuller particulars reſpecting this extraordinary cure, I am happy to be able to add the following teſtimonies:

A Letter to Dr. Thornton, from Mr. Carpue, Surgeon at the York-Hoſpital.

DEAR SIR,

I have ſeen Mr. M'Kennon, and have examined his leg, and think the cure you have wrought on it is indeed very aſtoniſhing. When he firſt came under my care, he laboured under an immenſe ulceration, extending from the external ancle of the right leg, which reached as high as the junction of the tendons of the gaſtrocnemii and ſolæus muſcles. At this period the tendons of the peronæal muſcles had ſluffed, and in conſequence I applied charcoal, which produced very conſiderable good; but upon mentioning this to a friend, who knew the caſe well*, he ſaid, "It was immaterial what remedy I uſed, for it was a caſe in which he was certain nothing would prove effectual." After this I applied the diluted nitrous acid, and ſeemingly with advantage; but being obliged to go into the country, I left him under the care of another ſurgeon; and when I ſaw him, after an abſence of ſix weeks, I found the ſore in a very unfavourable condition; and for four months I uſed different applications, but to no purpoſe, and I conceived the caſe now to be perfectly incurable, and as ſuch mentioned it to you when I had the pleaſure of meeting you at Mr. Heaviſide's: and I then propoſed him to you for the trial of the oxygen air, as his caſe, if ſucceſsful, would prove moſt decidedly its efficacy; for, in the multitude of ſore legs I have at-

* We believe this to be Dr. Bailey, phyſician to St. George's Hoſpital.

tended,

tended, I muſt acknowledge I never ſaw a worſe caſe, old Weſt-India ſores excepted.

I have the honour to be, Sir,
Your obedient ſervant,
J. C. CARPUE.

Having referred Mr. M'Kennon to Mr. Spencer, a ſurgeon in Charlotte-ſtreet, Fitzroy-ſquare, who adminiſters the pneumatic remedies, I received the following letter:

A Letter to Dr. Thornton from Mr. Spencer.

SIR,

I here encloſe the treatment and the progreſſive cure of the ulcer which occupied the external ancle of the right leg of Mr. M'Kennon. For ſix weeks, by your direction, he daily took a gallon of oxygen air, mixed with four times that quantity of atmoſpheric air. The ulcer diſcharged properly, but ſeemed to heal very ſlowly: in conſequence I gave him a double doſe, and after a fortnight it produced very feveriſh ſymptoms, when he took by your order ſome purgatives, and then he reſumed his uſual doſe of ſuperoxygenated air daily, until the ulcer, diminiſhing by degrees, was at laſt completely healed, there being no diſcharge, the whole cicatriſing, and the new-formed ſurface looking extremely healthy. During this period no particular application or dreſſings were made uſe of by me, nor any medicine directed by you, but what before he ſaid he had taken gallons of; ſo that I attribute his extraordinary cure entirely to the efficacy of the oxygen air. Happy in being able to give my teſtimony to ſo remarkable a caſe, I have the honour to be, dear Sir,

With the profoundeſt reſpect,
Your obedient humble ſervant,
T. SPENCER.

REMARKS BY DR. THORNTON.

As Mr. M'Kennon took bark, ſome of the Faculty may not be willing to give to the oxygen air the merit in this cure; I will therefore endeavour to ſtate ſhortly my reaſons for attributing every thing to this new remedy.

1. The

1. The operation of bark had been before tried; he had taken, he faid, gallons of it.

2. When I firft faw him, the fore, and mufcles furrounding it, were wholly infenfible; he did not feel a needle piercing them, nor could he perceive even the corrofive operation of cauftic.

3. After inhaling the vital air but a few days, fenfibility was reftored, as both Mr. M'Kennon and Mr. Carpue witneffed.

4. Having cleanfed the wound, it would remain dry; but even whilft inhaling the vital air, the whole furface was immediately covered with a fine dew, as Dr. Monro and others witneffed.

5. To fhew the progrefs of amendment whilft inhaling the fuperoxygenated air, I am happy to be able to lay before the philofophic world the following teftimony of an impartial obferver, Dr. Douglas of Baliol-College, Oxford:

"Towards the middle of March 1798, I firft faw Mr. M'Kennon. He had then a large and very foul ulcer, extending fome inches above the right ancle. From that time to the prefent (April 30) I have repeatedly feen him, and *each time could not poffibly fail to be fenfible of a moft manifeft improvement*. At prefent *the ulcer is diminifhed at leaft one-half in fize fince I firft faw him*; the edges have a fine healthy appearance, and its general furface is aftonifhingly altered for the better."

6. When the oxygen air was left off, the fore remained ftationary, and vifibly improved when he again refumed it.

7. My ftrongeft argument, however, is the fuccefs in this and in other cafes equally defperate.

Mr. Munt had been before cured of a fore leg of eighteen years ftanding.

Mr. Atwood was cured of a fore leg of two years. When I afked Mr. Cruikfhank whether it was true he had condemned the leg? he anfwered, with his ufual emphafis, "I not only condemned his leg, but his life; for he was of fo wafhy a conftitution that he could not have loft the one without the other."

Next the cure of Patterfon. When I fent him to fhew

his leg to Mr. Cruikſhank, which ſtill poſſeſſes the marks of numerous ulcers, ſeeing varicoſe veins, this experienced ſurgeon ſaid: "Tell Dr. Thornton that he is miſtaken if he ſuppoſes he has made a permanent cure; for varicoſe ulcers were never cured without an operation, which, if he wiſhes, I will perform." The man, frightened at firſt, and then aſtoniſhed, replied: 'Sir, I have been cured perfectly now theſe three years.'—"That alters the caſe," anſwers this diſtinguiſhed anatomiſt; "then tell Dr. Thornton that he has performed a moſt wonderful cure."—Patterſon ſtill continues well; nor does there ſeem the ſmalleſt cauſe to ſuſpect a relapſe.

The cure of Mr. Wilkinſon *, who had a ſore leg twelve years, is not leſs extraordinary. In this caſe I obſerved a peculiar phenomenon, alone explicable by the operation of the oxygen air. The fingers of both hands at their ends looked very red, as red as raw meat, were ſwollen, and felt very painful. The ſame was mentioned to me in private converſation by Dr. Beddoes in a patient of his, who, finding an aſthma relieved by a ſmall doſe of vital air, took as much as he could at one time, produced a fever, and this ſame phenomenon I have juſt mentioned above.

This leads me to repeat an obſervation I have before often expreſſed, that oxygen air promiſes to be an uſeful remedy in ſore legs; for why have we not ſore arms? The nearneſs of this part to the heart ſeems to be the only philoſophic reaſon; and therefore a direct powerful ſtimulus to the heart, as oxygen air, promiſes the moſt certain good, aided by the invigorating effects of bark, ſteel, and other tonic medicines; not but that I would adviſe, where it can be properly done, as in hoſpitals, trials to be made with the vital air without medicine, to prevent all cavil; although it is undoubtedly unimportant to the ſufferer by what means he is treated, ſo that he is *but cured*; and, until the contrary is proved, I ſhall ever think, that medicines, judiciouſly employed, certainly cannot *impede* the operation of oxygen, but may *aſſiſt*.

I now take my leave of the philoſophic world, not from dearth of materials, for I have cures by me yet more im-

* The caſes of Mr. Munt, the Rev. Mr. Atwood, Patterſon, and Mr. Wilkinſon, are related in Dr. Beddoes's Conſiderations on Factitious Airs.

portant

portant than thoſe before related by me, that deſerve, I think, to be recorded; and daily experience, in a large public charity*, where the airs are adminiſtered under my direction, would afford me other frequent opportunities; but according to my promiſe, mentioned in my firſt communication, I was to continue writing on this ſubject only until the eſtabliſhment of the Pneumatic Inſtitution under Dr. Beddoes, which I am happy to announce, and that ſhortly the public may expect a periodical quarterly publication, relating chiefly to this important inveſtigation, from the maſterly pen of that eminent phyſician.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

GERMANY.

THE Electoral Academy of Sciences at Manheim has propoſed the following queſtion as the ſubject of a prize for the year 1801:—

"Are the azotic (*ſlick*) gaſes, which are produced from ſo many totally diſſimilar ſubſtances, and in ways ſo different, exactly the ſame in all their chemical properties and baſes (ſimple azotic gaſes,) with that of the atmoſphere? and has the nitrous acid the ſame azote for its acidifying baſe, as the atmoſpheric azotic gas?

"The partizans of the antiphlogiſtic doctrine ſeem to admit both, but without ſatisfactory proof. 1, Since, for want of ſufficiently accurate examination, they admit in each of theſe gaſes all the known properties of azotic gas, becauſe they deſtroy animal life, extinguiſh flame, and manifeſt no acid properties: but, 2, after all their analytic and

* The Mary-le-bone General Diſpenſary.

ſynthetic

ſynthetic proofs, they have ſtill left well-founded doubts, (*a*) whether the electric ſpark, in its paſſage through oxygen or azotic gas, does not itſelf undergo a chemical decompoſition, and furniſh the baſis of the nitrous acid; (*b*) and how, by the ſame means, (a red heat, and the electric ſpark,) the nitrous acid is decompoſed into oxygen and azotic gaſes, and can be again recompoſed from them; and, (*c*), ſince oxygen and azotic gaſes have ſo great an affinity for each other in the atmoſphere, why, when the former is added in a ſufficient quantity, imperfect nitrous acid is not immediately produced, as is the caſe when oxygen gas is added to the azote in nitrous gas, by which perfect nitrous acid is immediately produced?

"The papers on this ſubject, written either in Latin or French, muſt be tranſmitted before the 1ſt of November 1800, to M. I. Kenedy, ſecretary to the academy, with the name of the author, in a ſealed note, and any motto choſen at pleaſure. The prize is a gold medal, of the value of 50 ducats."

HOLLAND.

The Dutch Society of the Sciences at Haerlem propoſed in 1793, and afterwards in 1796, the following queſtion: "What light has Lavoiſier's ſyſtem, and his method of examining organic ſubſtances, furniſhed towards a more accurate knowledge of the human body?" But, as no ſatisfactory anſwer was received, they have propoſed it again in the following manner:—

I. As a great number of new diſcoveries have been made ſince the queſtion was firſt propoſed, and as the circumſtances of it have thereby acquired more extent than to admit of their being properly comprehended in a ſingle treatiſe, the Society have reſolved to divide the different objects of it into three new queſtions for the preſent year, and to fix the period of receiving anſwers at the 1ſt of November 1800.

1. What light has the new chemiſtry thrown on the phyſiology of the human body?

2. How far has the light, thrown on the phyſiology of the human body, contributed to a better knowledge than before of the nature and cauſes of certain diſeaſes; and what uſeful

 conſe-

confequences, more or lefs confirmed by experience, can be deduced from it in regard to the practice of medicine?

3. How far has the new chemiftry contributed to afford an accurate idea of the mode of action of different internal and external medicines, which have been long ufed, or only lately recommended? And, what advantages can arife from a more accurate knowledge of this point in regard to the treatment of certain difeafes?

As fome learned men have introduced hypothefes built on too weak a foundation, in regard to the application of the principles of the new chemiftry to phyfiology, pathology, and therapeutics; and as this is highly prejudicial to the progrefs of thefe fciences, to which the new chemiftry, however, promifes fo much light, if, according to Lavoifier's rule, we admit nothing in chemiftry, or the employment of chemical principles, but what is founded on decifive experiments, the Society requires, that thofe who are inclined to anfwer thefe queftions will make an accurate diftinction between what is proved, and what is merely hypothetical; and that, in regard to hypothefes, the candidates will confine themfelves to a bare mention of them, and of the few grounds on which they reft; becaufe the principal point which the Society wifhes to obtain is, that thofe who follow the medical or chirurgical profeffion in Holland, and who are not yet fufficiently acquainted with the progrefs of the new chemiftry, and its application, on well-founded principles, to phyfiology, pathology and therapeutics, will procure fuch works as may be beft calculated to inform them what light the new chemiftry has actually thrown on thefe fciences; and what facts are founded on too weak grounds; and what have been too rafhly adopted, or are ftill too doubtful to be depended on. Each of thefe papers will be examined feparately: thofe, therefore, who wifh to anfwer more than one queftion, muft fend a paper for each.

II. The Society requires a plan, capable of being carried into execution, for rendering productive the large uncultivated diftricts of the republic, particularly in Guelderland, Overyffel, Drenthe, and Dutch Brabant.

III. The

III. The following prize questions are again proposed :—

1. A natural history of the whale; in order to furnish hints for its being more easily discovered and caught, and afterwards converted to use. This question to be answered before the 1st of November 1802.

2. What has experience taught in regard to the use of certain animals which in the Netherlands appear to be hurtful; and what means are to be employed for extirpating them? For this question no period is defined.

3. What indigenous plants, the virtues of which have been hitherto unknown, might be employed in the apothecaries shops in Holland to supply the place of foreign medicines? The virtues of them must be established, not by foreign testimony, but by the testimony of natives of the country.—The time for answering this question is indefinite.

4. What indigenous plants, not yet employed, might be introduced into use as good and cheap food? And, what foreign nutritive plants might be cultivated for the same purpose?—No definite time.

FOSSIL WOOD FOUND AT A GREAT HEIGHT.

In a paper lately read before the French National Institute, it appears that C. Villars, Professor of Natural History, of Grenoble, saw, near a glacier in the department of Isere, some fossil wood buried among turf at the height of 2320 metres above the level of the sea, and 850 metres above the most elevated line at which wood grows at present. The mountain on which this interesting discovery was made, is that of Lans, in the canton of Oisans. The trees found there are, mountain-ash, birch, and the common larch. The roots and part of the trunks can be plainly distinguished. The last of these trees does not grow at present in the neighbourhood.—The author of this memoir ascribes the greater degree of cold, which now prevails on these mountains, to two principal causes: first, the valleys becoming deeper, which has changed the elevation of the summits in regard to their bases and the surrounding countries: the second is the destruction of the ancient forests, which had gradually extended themselves to great heights, but which, when once destroyed, cannot grow

up again at the same heights, because the trees are deprived of that mutual shelter which they afforded to each other.

NEW THEORY OF RESPIRATION.

Professor Herholdt read lately, before the Academy of Sciences at Copenhagen, a memoir respecting some experiments made by him and M. Rafn on living animals, in order to discover the mechanism of respiration; having in view, at the same time, the cure of wounds in the breast. The professor shewed that the best authors on surgery have hitherto explained the mechanism of respiration in a manner diametrically opposite to what it really is; so that, by applying their theory to the cure of wounds in the breast, they have followed a method altogether false. According to his experiments the lungs have not, as has been maintained, an expansive force peculiar to them, but the movement is performed by the action of the diaphragm, to which sufficient attention has not hitherto been paid. When there are wounds in the breast, the atmospheric air enters by them on inspiring into the cavities of the thorax, and issues on expiring. This has been proved by experiments made on horses, dogs and cats.

M. Herholdt and Rafn, in examining the manner in which the frog breathes, remarked, that this animal is without a diaphragm, and that its' lungs at the same time have no expansive force; but that a small membrane, by means of which it can shut its mouth hermetically, discharges the function of the diaphragm; so that, when it is prevented from shutting its mouth by inserting into it a small rod, the animal dies in a few minutes, because it is no longer able to breathe. When it is suffered to shut its mouth before it is entirely dead, or when it is only in a state of asphyxia, it soon recovers. If a frog be deprived of this membrane, by cutting it entirely off, or only in part, so that its mouth can no longer be hermetically shut, it expires in a longer or shorter time according to the size of the aperture made: on the first view it appears very paradoxical that man, as well as the greater part of animals, loses his life by not being able to breathe when his mouth and nose are shut, and that the frog dies because it cannot breathe when its mouth is opened. The explanation of

of this phenomenon, however, is eaſy when we recollect that the lungs have no expanſive force. In conſequence of this new theory, M. Herholdt has ſucceeded in curing very dangerous wounds made in the breaſts of dogs.

The above experiments, which were communicated to the Philomatic Society at Paris, by M. Manthey, were repeated with ſucceſs, by the commiſſioners of that Society, on frogs and ſalamanders. If a gag be put into the mouth of one of theſe animals, ſo as to prevent it from ſhutting its mouth, it dies at the end of half an hour. The reſpiration is performed as follows:—The mouth being abſolutely ſhut, the frog dilates its throat, and the air ruſhes in by the noſtrils; it afterwards contracts its throat, and the air penetrates to the lungs, becauſe, no doubt, there is a ſmall valve in the noſtrils which prevents it from eſcaping by the ſame way that it entered; for the membrane, which the Daniſh authors aſſert they obſerved in the mouth, could not be ſeen by the commiſſioners. Lizards and ſerpents, which have ribs, breathe like other animals; and a forced opening of the mouth does not kill them.

MEDICAL PNEUMATIC INSTITUTION.

All who wiſh well to the intereſts of humanity will rejoice to learn that this inſtitution, as uſeful as novel, is at laſt ſo far eſtabliſhed that there is every reaſonable ground to believe the objects of it will now receive that ample and fair inveſtigation their importance demands. The learned, ingenious, and meritoriouſly perſevering founder of it, has already publiſhed a *Notice of Obſervations made at the Inſtitution,* which contains ſome highly intereſting remarks on the effects of a gas not before applied to medicine, but which promiſes, in ſkilful hands, to be one of the ſafeſt and moſt powerful agents hitherto diſcovered.

Mr. Davy, ſuperintendant of the inſtitution, than whom it would hardly be poſſible to find one better qualified for the ſituation, having made ſome experiments on *dephlogiſticated nitrous gas,* ſo named by its diſcoverer Dr. Prieſtley, which proved that its compoſition, properties, and mode of action, had been miſtaken by the lateſt experimenters, was induced

to inhale it. Dr. Beddoes relates the circumſtances in the following words:

"The firſt inſpirations of the gas produced giddineſs, fulneſs of the head, and, in ſhort, feelings reſembling thoſe of incipient intoxication, but unaccompanied by pleaſurable ſenſation. At this next experiment I was preſent. The quantity was larger, and the gas more pure. The ſcene exhibited was the moſt extraordinary I had ever witneſſed, except in the caſe of that epileptic patient, whom I have deſcribed (*Conſiderations on Airs*, part iv. p. 13.) as agitated, in conſequence of the reſpiration of oxygen gas, with a long ſucceſſion of the moſt violent movements. The two ſpectacles differed, indeed, eſſentially in one reſpect. In the former every thing was alarming: in the latter, after the firſt moments of ſurprize, it was impoſſible not to recognize the expreſſions of the moſt ecſtatic pleaſure. I find it entirely out of my power to paint the appearances, ſuch as they exhibited themſelves to me. I ſaw and heard ſhouting, leaping, running, and other geſtures, which may be ſuppoſed to be exhibited by a perſon who gives full looſe to feelings excited by a piece of joyful and unlooked-for news. As in the caſe of the epileptic patient, *no wearineſs or depreſſion followed*; ſo, in this caſe, *no exhauſtion or languor or uneaſy feeling took place*. The experiment Mr. Davy has very frequently repeated, and generally with the higheſt pleaſurable ſenſations; and, except under particular circumſtances, with conſiderable muſcular exertions, which have not in any inſtance been ſucceeded by fatigue or ſadneſs."

A number of perſons afterwards inhaled the ſame gas: the following extracts will convey ſome idea of the very ſingular effects produced by it:

"Mr. J. W. Tobin (after the firſt imperfect trials), when the air was pure, experienced ſometimes ſublime emotions, with tranquil geſtures; ſometimes violent muſcular action, with ſenſations indeſcribably exquiſite; no ſubſequent debility—no exhauſtion.—His trials have been very numerous. Of late he has felt only ſedate pleaſure. In Mr. Davy the effect is not diminiſhed.

Mrs.

"Mrs. Beddoes—Pretty uniform pleaſurable ſenſations—propenſity to muſcular exertion, could walk much better up Clifton Hill—has frequently ſeemed to be aſcending like a balloon, a feeling which Mr. Burnet ſtrongly expreſſed.

"Mr. James Thomſon. Involuntary laughter—thrilling in his toes and fingers—exquiſite ſenſations of pleaſure—a pain in the back and knees, occaſioned by fatigue the day before, recurred a few minutes afterwards. A ſimilar obſervation we think we have made on others; and we impute it to the undoubted power of the gas to increaſe the ſenſibility, or nervous power, beyond any other agent, and probably in a peculiar manner.

"Mr. Stephen Hammick, Surgeon of the Royal Hoſpital, Plymouth. In a ſmall doſe, yawning and languor.—It ſhould be obſerved, that the firſt ſenſation has often been diſagreeable, as giddineſs; and a few perſons, previouſly apprehenſive, have left off inhaling as ſoon as they felt this.—Two larger doſes produced a glow, unreſtrainable tendency to muſcular action, high ſpirits, and more vivid ideas.

"Mr. Robert Southey, for many hours after the experiment, imagined that his taſte and ſmell were more acute, and is certain that he felt unuſually ſtrong and cheerful. In a ſecond experiment, he felt pleaſure ſtill ſuperior—and has ſince poetically remarked, that he ſuppoſes the atmoſphere of the higheſt of all poſſible heavens to be compoſed of this gas.

"Robert Kingleake, M.D. Additional freedom and power of reſpiration, ſucceeded by an almoſt delirious but highly pleaſurable ſenſation in the head, which became univerſal, with increaſed tone of the muſcles. At laſt an intoxicating placidity abſorbed for five minutes all voluntary power, and left a cheerfulneſs and alacrity for ſeveral hours. A ſecond ſtronger doſe produced a perfect *trance* for about a minute; then a glow pervaded the ſyſtem. The permanent effects were, an invigorated feeling of vital power and improved ſpirits. By both trials, particularly by the former, old rheumatic feelings ſeemed to be revived for the moment.

"Mr. Wedgwood, after breathing ſome time, threw the bag from him, kept breathing on laboriouſly with an open mouth, holding

holding his nose with his left hand* without power to take it away, though aware of the ludicrousness of his situation—all his muscles seemed to be thrown into vibratory motion—he had a violent inclination to make antic gestures—seemed lighter than the atmosphere, and as if about to mount. Before the experiment, he was a good deal fatigued after a very long ride, of which he permanently lost all sense. In a second experiment nearly the same effects, but with less pleasure. In a third, much greater pleasure."

The cases in which its effects were prejudicial are also stated with great candour. These were of the hysterical kind; but the consideration of the whole of these phenomena led to a happy application of the gas to the cure of palsy, and of diseases proceeding from a defect of nervous energy; and several instances of success are stated, which are well deserving of attention.

"We desire further," says the author, "to try the effects of our new agent in palsy, and in the various cases of *true* deficiency of nervous power, which we have well learned to distinguish from cases of relative defect of irritable power. As the limits of the efficacy of every remedy ought to be determined, we shall not shrink from any case, by reason of its inveteracy. We intend to oppose our Nepenthe to the equable decay induced by time and intemperance; and we hope to palliate some of the evils of extreme old age itself.

"We are emboldened by experience to pledge ourselves for the safe employment of the gas. We shall, indeed, be sadly disappointed if it do not sometimes prove the most delicious of luxuries, as well as the most salutary of remedies. In saying this, it may be allowed me to suggest to those, who have not attended to the tenor of my opinions, that I now for the first time venture to hold forth these hopes. However urgently I may have recommended the investigation, my language, with regard to its issue, has always been, that *I would not answer for the discovery of a gaseous remedy in any denomination whatever of disease. That natural or forced decay may be repaired, and the faculty of pleasurable sensation renovated,*

* This was practised in all the experiments.

is

is now no longer a mere conjecture supported by loose analogies—we see the strongest probabilities daily accumulating in favour of the opinion: It must only be remembered, that so desirable a change cannot be effected by the agent applied in *any* manner to *any* constitution. It must be *properly* used in *proper* cases.

"Considering the present abundance of expert chemists, we cannot presume that others will not be able to prepare the gas perfectly without our instructions. Nevertheless, those who attempt to use it medicinally should be apprized that the utmost care is necessary in its preparation and employment. A deleterious, instead of a salutary fluid, as the author can attest from his own painful experience, may easily be obtained. Probably neither Dr. Priestley, nor the Dutch chemists, ever procured that which can be respired with safety. The difference, and its causes, will hereafter be pointed out.

"If, therefore, in consequence of that promptitude which journals afford, experiments tending to invalidate any part of the preceding statement should be published, we hope that its perfect fidelity will not on that account be doubted. The experimenter will be found to have employed a different agent from ours. This we have no hesitation in declaring that we shall be able to shew."

From the preceding short extracts, our readers will see what an interesting field is opened for the advancement of physiology and medicine; and we hope that some will be induced to assist an institution, which ought not to be confounded with ordinary charitable foundations. It is established for the sake of relieving from a species of distress, from which no degree of affluence can exempt—namely, that which arises from the imperfect state of medicine. Subscriptions are received by Mr. Couts, Banker, Strand, London; and by Mr. Savery, Banker, Bristol.

CHROMAT OF IRON.

C. Pontier, correspondent to the *Journal des Mines*, transmitted lately to the Cabinet of the House of Instruction, among other interesting minerals, a substance in an irregular mass, of a dark brown colour, having a metallic splendour

 and

and mean hardneſs, the ſpecific gravity of which was found to be 4·0326. C. Pontier found it in the department of Var, à la Baſtide de la Carrade, near Gaſſin, and conſidered it as the brown blende, to which indeed it has a conſiderable degree of reſemblance, except that its ſpecific gravity is far greater. This ſubſtance being analyſed in the laboratory of the mines by C. Taſſaert, was found to be chromat of iron, that is to ſay, a metallic ſalt formed by the combination of iron with the acid ariſing from the new metal diſcovered by C. Vauquelin, to which he gave the name of chrome. It ſeemed to contain, in 100 parts, 63·6 of that acid, 36 of iron; loſs, 1·4. Chemiſts may now flatter themſelves that chrome, which hitherto has been found only in the red lead of Siberia, in the ruby and the emerald, may be obtained in ſufficient abundance to enable them to ſubject it to new reſearches.

C. Vauquelin and Taſſaert, by continuing their experiments on this ſubſtance, have aſcertained the following facts:

1. It does not melt alone by the blow-pipe, but with borax, to which it communicates a green colour like that of the emerald.

2. It is ſoluble in the muriatic acid, but ſlowly, and in ſmall quantity. From its ſolution, which is of a greeniſh blue colour, it is precipitated white by alkalies.

3. It is ſoluble in oxygenated muriatic acid. From this ſolution, almoſt colourleſs, it is precipitated of a reddiſh brown by alkalies, and of an orange yellow by nitrat of lead.

4. It is not decompoſable by potaſh, or the carbonat of potaſh, without the aſſiſtance of a degree of heat which carries it to incandeſcence.

5. It may eaſily be reduced by the known means. It then gives an alloyed maſs, having only an external metallic aſpect, eaſily fuſed with borax; brittle, but hard as ſteel.

6. This maſs is exceedingly difficult to be decompoſed. It muſt be treated ſucceſſively and ſeveral times with potaſh, which combines with the chromic acid and diſſolves it, and with the muriatic acid, which diſſolves the oxyd of iron.

7. Other

7. Other experiments have proved the presence in this mineral of silex and alumine; so that C. Vauquelin and Tassaert believe it to be composed of the following substances:

Chromic acid	-	-	43
Oxyd of iron	-	-	34·7
Argil	-	-	20·3
Silex	-	-	2

They are of opinion also, that, the chromic acid being in sufficient quantity to saturate the oxyd of iron, this mineral is a triple combination of the chromic acid, the oxyd of iron, and argil.

8. The oxyds of chrome, or the chromic acid, may be employed in the porcelain manufacture. When pure, they give an emerald green, more beautiful than that of copper; and, mixed with lead or antimony, a canary-bird green. They may be employed also in painting, by separating the acid from the iron, and combining it afterwards with different metallic oxyds by double affinities.

METHOD OF CHECKING DECAY IN TREES.

The chestnut lives a long time, and often attains to an extraordinary size; but unfortunately the texture of its wood alters under certain circumstances: it becomes soft, falls into dust, a cavity is gradually formed in the heart of the tree, and this cavity, by the progress of the decomposition, becomes still larger; so that, at last, the trunk seems to consist of nothing but bark; and being too weak to support the branches, and resist the violence of hurricanes, it cannot long exist. It is by similar changes and decompositions of the ligneous principle, that trees, which have been growing for ages, are seen to perish in a very little time.

C. Chaptal, in travelling through different parts of France, and particularly the Cevennes and the department of Allier, observed, that a great number of chestnut-trees were hollow, and charred over the whole internal surface. He was told by the inhabitants of the country, that this process was employed to stop the progress of the caries, which otherwise would destroy the whole tree. When they observe that this

diſeaſe, which is very common and exceedingly fatal to the cheſtnut-tree, begins to make any progreſs, and to excavate the trunk, they collect heath and other vegetables, and ſet fire to them in the cavity, till the whole ſurface is completely charred. It happens very rarely that the tree periſhes by this operation, and it is always found that this remedy ſuſpends the effect of the caries. It is practiſed with the ſame ſucceſs on the white oak. By comparing the effects of the cautery on the human body, in analogous caſes of degeneration, we perceive a new ſimilarity between diſeaſes which affect the living organiſed beings of the two kingdoms, and between the remedies by which they may be checked.

ON THE GRAVITY OF TUNGSTEN.

The difficulty of bringing tungſten to complete fuſion has hitherto prevented the ſpecific gravity of this new metal from being with certainty determined. Some fixed it at 17·6, according to the experiments of the brothers d'Elhuyar; but many could not believe it to be ſo conſiderable. C. Guyton lately obtained a well-formed button, of the weight of 25 grammes, in a three-blaſt-furnace, where the intenſity of the fire may be carried to about 185° of Wedgewood: but this button having broken by the preſſure of the vice, into which it was put in order to be ſawn, there was diſcovered at its centre a part not agglutinated, which, by expoſure to the air, ſpeedily aſſumed a purple colour, ſimilar to that which the beſt fuſed tungſten, of ſuch a degree of hardneſs as to render a file brilliant, exhibits on its ſurface. It reſults, from theſe experiments, that the ſpecific gravity of the fuſed portion, ſeparated from that which was only fuſed imperfectly, was 8·3406; that the infuſibility and brittleneſs of this metal leave no other hopes of applying it to the arts, though there are abundant mines of it in France, but by mixing it with other metals, or by the property diſcovered in its oxyds of yielding fixed colours, and fixing vegetable colours.

FLUAT OF ARGIL.

C. Vauquelin has received from Denmark a white lamellated mineral brought from Greenland, which proves to be

 fluat

fluat of argil, an earthy neutral ſalt never before found in a natural ſtate.

ASTRONOMY.

C. Lalande has lately preſented to the National Inſtitute an account of his obſervation of the laſt oppoſition of Mars, with the calculations on that ſubject; and by comparing it with that of 1790, he has found that only 58 ſeconds are to be deducted from the place of the aphelion of Mars, employed in the laſt edition of his Aſtronomy. He announces a large work on this planet by C. Lefrançais-Lalande, his nephew.

C. Lalande has alſo given the calculation of eclipſes of the ſun or ſtars obſerved for ſome years, to deduce from them the poſition of different cities. He has found Hamburgh to be 30′ 9″ from Paris; Cobourg 34′ 30″; Mulheim 21′ 20″; Halle 38′ 28″; and Konigſberg 1ʰ 12′ 35″.

MONUMENT TO LINNEUS.

A monument has lately been erected in the cathedral of Upſal, to the memory of the great Linneus. It conſiſts entirely of the porphyry of Elfwedal; is properly a pedeſtal in the form of an altar, the ſteps of which are of the brown ſtone of Oeland; and ſupports a medallion containing a buſt of Linneus. The following is the inſcription:

CAROLO A LINNE,
BOTANICORUM PRINCIPI,
AMICI ET DISCIPULI.
M,DCC,XCVIIII.

SUBSTITUTES FOR COFFEE.

Count von Burgſdorf has lately laid before the Royal Academy of Sciences at Berlin, ſamples of coffee prepared from the ſolid parts of the beet-root, after the ſaccharine juice has been expreſſed.

After the moſt careful examination of all the ſubſtitutes for coffee hitherto employed, the Academy of Sciences at Peterſburgh has made known that the acorn is the beſt, as it poſſeſſes, when proper means are uſed to communicate to it the oily properties, all the requiſite qualities of coffee. To communicate theſe oily properties, the following proceſs is recommended:

commended: When the acorns have been toaſted brown, add freſh butter, in ſmall pieces, to them while hot in the ladle, and ſtir them with care, or cover the ladle, and ſhake it in order that the whole may be well mixed. By theſe means you will obtain the beſt and moſt harmleſs ſubſtitute for coffee hitherto employed.

SUBSTITUTE FOR TINCTURE OF BARK.

Dr. G. F. C. Fuchs, teacher extraordinary of medicine at Jena, has prepared from the ripe fruit of the horſe cheſtnut, *Aeſculus hippocaſtanum* LINN. when diveſted of the huſks, an extract, which, according to his experiments, may be uſed, perhaps, inſtead of the expenſive *extractum chinæ*, ſince the bark of this tree has been long known as a ſubſtitute for cinchona.

SUBSTITUTE FOR GALLS.

A German apothecary, named Trömer, has lately diſcovered, that the excreſcences or knots on the roots of young oaks may be uſed as a ſubſtitute for galls. Theſe excreſcences are produced, in the ſame manner as the galls, by an inſect, which, after pricking them, depoſits its eggs in them. With vitriol of iron, in the ſame proportion as galls, they give a beautiful black ink, and may be uſed alſo in dyeing. In the ſpring, theſe excreſcences may be found in great numbers on the ſmall roots of the oaks, particularly on the ſouth ſide, often about a hand's-breadth below the earth. Thoſe found in ſummer have, for the moſt part, ſmall apertures, capable of admitting a moderate-ſized needle, but they are no longer filled with eggs. At this period they are more woody, and not ſo good; and therefore they ought to be collected in the ſpring.

TRAVELS IN AFRICA.

Mr. Horneman, whom we ſome time ago announced to have ſet out with the view of exploring the interior of Africa, has written a letter to Sir Joſeph Banks, from Tripoli. He had travelled from Cairo, in Egypt, through the Lybian Deſert, to Fezzan, the largeſt Oaſis in the Great Sahara; a route hitherto unexplored by any European, whoſe travels have

have been communicated to the public. In the journey from Cairo to Fezzan, he halted at Sewah, which, from the notices of Mr. Brown, fome months ago, had been clearly afcertained to be the Oafis of Ammon.

Mr. Horneman's new obfervations, made at his leifure on the fpot, now place this matter beyond all manner of doubt. Mr. H. was too late this feafon for the caravan that goes from Fezzan to Soudan, comprehending under that name Hourfes, Cafhnou, Burnou, the great kingdom near the Niger. Meanwhile he has fent from Tripoli, by another conveyance, not yet arrived, the journal of his prefent travels; and there is every reafon to hope that he will accomplifh his great undertaking of vifiting the unknown central regions of Africa, efpecially from the following occurrences mentioned in his letter:—He was followed from Sewah by a large party fent to feize him, on fufpicion of his being a French fpy. But his manners and behaviour were fo completely Moflem, and he approved himfelf fo thoroughly mafter of the Koran, that he was releafed with bleffings and alms as a good muffulman, and fent forward on his journey.

CEMENTING OF BROKEN GLASS.

C. Pajot, of Charmes, lately tranfmitted to the Philomatic Society and the National Inftitute, fmall bits of glafs of different qualities, which he had joined and foldered fo firmly that the glafs would rather break clofe to the joining than in the actual place. The form of the fracture does not at all prevent the operation. The line of junction is fcarcely difcernible, and in fome places not vifible. C. Pajot has not made his procefs known.

C. Swediaur informed the Society, that a perfon named Hollenweger, about twelve or fourteen years ago, had in his prefence, and that of Lavoifier and Meunier, performed fome experiments, by means of which he joined in a folid manner, fo as to make the junction hardly vifible, fragments of blown glafs.

C. Chaptal faid alfo that fome perfon had fhewn him, about fifteen years ago, a glafs bottle, the neck of which was fo perfectly foldered to the glafs ftopper, that, when cut through, the circle of junction was fcarcely perceptible. The

bottle

bottle contained liquor of flints, and had lain on its side for a long time. "After seeing this fact, said Chaptal, I conceived the possibility of soldering together two plates of glass. I explained my ideas on this subject, and shewed the bottle, in my public lectures; and I am of opinion, that, by gradually withdrawing the dissolving alkali, it might be possible to unite such plates."

Before we dismiss this article, we beg leave to remark, that there appears to be nothing so wonderful in joining broken pieces of glass, in the way above described, as to justify Pajot in concealing the process. A little reasoning will lead any one to it. All that is necessary is to interpose, between the parts, a glass ground up like a pigment, but of easier fusion than the pieces to be joined, and then exposing them to such a heat as will fuse the cementing ingredient, and make the pieces agglutinate without being themselves fused. A glass fit for the purpose of cementing broken pieces of flint glass, may be made by fusing some of the same kind of glass, previously reduced to a powder, along with a little red lead and borax, or with the borax only.

DIED.

At Edinburgh, on the 6th inst. Dr. Joseph Black, one of the Physicians to his Majesty for Scotland, Professor of Chemistry in the University of Edinburgh, and a Member of the different respectable Medical and Literary Societies in Europe. The noble science of Chemistry has received many valuable improvements and elucidations from the genius and industry of the learned Dr. Black, which will transmit his name with honour to the latest posterity.

His papers were found in such perfect order by his executors, that we understand they intend speedily to publish them, with a life of the author.

Errata in our Last.

In the description of Mr. Howard's Furnace, last line, (p. 192,) for *Windsor* read *fire* bricks.

Page 198, line 26, for *typani* read *tympani*.

In the description of the monument to Count Rumford, p. 206, line 17, read

At the creative glance of Charles Theodore,
Rumford, the friend of mankind, &c.

THE

PHILOSOPHICAL MAGAZINE.

JANUARY 1800.

I. *A Communication from* George Pearson, *M. D. F. R. S. &c. Physician to St. George's Hospital, &c. concerning the Eruptions resembling the Small-Pox, which sometimes appear in the Inoculated Vaccine Disease.*

FOR reasons, the explanation of which would lead into too long a detail, and which, indeed, cannot be given with perfect propriety in this sheet; I feel myself compelled to publish some observations concerning the eruptions which appear, in some instances, in the cow-pock by inoculation.

Although the new inoculation in the present year has been, I think, sufficiently extensive to manifest the advantages of it over that for the small-pox, so that it is not likely to be ever totally laid aside; yet the unexpected appearance of eruptions has inclined many persons to be of opinion that no beneficial consequences can be produced by this practice, or, at least, that such consequences at best seem to be problematical. It may be useful to observe, that some of the Advocates for the cow-pock inoculation contend, that eruptions are never produced by it; accordingly, they assert, that, in these eruptive cases, the disorder was not the cow-pock, but the small-pox; the variolous poison having either been inserted inadvertently, or the constitution having been af-

fected by it caſually. To juſtify what is advanced, it is incumbent on the Aſſertors either to prove that ſuch errors have been committed, or at leaſt they ought to be able to oppoſe equally extenſive experience to that of the adverſe party: for no ſound reaſoner will conſider opinions, which are only founded in conjecture, to be demonſtrated truths. I will not, however, take upon myſelf the unrequired taſk of attempting to vindicate others from the above charges; but I ſhall only perform a duty in ſtating the reſult of my own experience with regard to the point in queſtion; conceiving that by this means the truth may be brought to light, if aided by the evidence from experience of future inquirers.

In the courſe of my practice the latter end of February, and in March following, I diſtinctly recollect four caſes in which I firſt ſaw eruptions from the vaccine inoculation reſembling ſo much thoſe of the ſmall-pox, that I ſhould not have heſitated to conſider them as belonging to this diſeaſe, if I had not excited them by a different poiſon from the variolous. I obſerved, however, at that time, ſome appearances of theſe eruptions different from thoſe which uſually occur in the ſmall-pox. Almoſt all the eruptions, in the ſtage of deſiccation, afforded ſhining, ſmooth, black or reddiſh-brown ſcabs; very few of them having previouſly ſuppurated. Finding, in two other inſtances, that the matter from the inoculated puſtule of theſe patients produced a ſimilar eruptive diſorder, and alſo the ſame being the event in the practice of two or three of my correſpondents, whom I had furniſhed with matter from the above eruptive caſes, I from that time uſed matter only from the caſes in which no eruptions appeared. After this precaution, no eruptive caſes reſembling the ſmall-pox occurred in my practice during the whole of laſt ſummer and the preſent winter. I ſay, no caſes occurred reſembling the ſmall-pox; but certainly eruptions, in number from a ſingle one to about a dozen, which were large, red, hard pimples, with little or no lymph, and never with any pus, occurred, probably, in one caſe out of twenty or thirty. Theſe ſpots, ſo unlike the ſmall-pox, produced no trouble; and were of ſuch a ſhort duration, that, when I ſpeak of eruptions, I do not include them in the number; I include in the account thoſe only in which the

eruptions refembled the fmall-pox: nor do I reckon among the eruptive cafes thofe in which, now and then, a rafh broke out about the 14th day after inoculation; and which was as troublefome as the *Urticaria*. My experience, then, with refpect to the cafes of eruptions being diminifhed in number by avoiding inoculation with matter of fimilar eruptive cafes, coincides with Dr. Woodville's; and confirms what he has already fo ufefully communicated to the public. It was obvious to fufpect, on the firft occurrence of the eruptive cafes, that variolous matter, in an unobferved way, but from fources which could not even be conjectured, had been introduced into the conftitution inftead of the vaccine poifon. This conjecture, in fpite of the cleareft evidence of fenfe refpecting the nature of the matter ufed, received fome countenance from the non-appearance of eruptions as above ftated; but, from the occurrence of fuch cafes, in the practice of other Inoculators, in the laft autumn and this winter, I think it is very unreafonable to doubt any longer, that, either on account of peculiar ftates of the human animal œconomy, or on account of fome co-operating agents, the genuine vaccine poifon does now and then produce a certain variety of the cow-pock, characterifed by the appearance of puftules, like thofe of the variola. I have good evidence alfo to fhew, that even in the hands of thofe very Inoculators, who a little time ago would not allow that the vaccine poifon could produce eruptions, fuch cafes have lately occurred.

In the month of October laft I inoculated a child two years of age with the vaccine poifon. The original matter, which had produced this matter, I took from the cow in March laft; fince which time the vaccine difeafe had been excited by it, in my hands, in a great number of patients. The vaccine difeafe took place with the ufual appearances, in the inoculated part, and affected the whole conftitution in the ordinary manner; but a few eruptions broke out on the fecond or third day after the flight fever; they were, however, only the red large pimples above mentioned, not at all like the fmall-pox. Mr. Keate carried matter from this child to Brighthelmftone, where Mr. Barret inoculated *two* children, who took the difeafe; and from one of thefe Mr. Keate ino-

culated *three* children. They all had the uſual fever about the eighth day, and all had a number of eruptions, except one, which had only five or ſix, and theſe dried on the fifth day. This laſt caſe was probably that which Mr. Keate informs me had, in the inoculated part, the genuine vaccine puſtule; but in all the others Mr. Barret obſerved that in the inoculated part the puſtule was ragged at the edges, and flat, more reſembling the variolous puſtule. Matter from theſe patients was ſent to Petworth, where Mr. Andre informs me he inoculated with it *fourteen* children. They all took the diſeaſe, and had eruptions like the variolous. *Three* children at the breaſt had from *three* to *twelve puſtules*. The remaining *eleven* children had from fifty to ſeveral hundred eruptions. The ſtate of the arms, and the characters of the puſtules of the inoculated part, are not mentioned. None of the above patients died, nor is any mention made by Mr. Keate, Mr. Barret, and Mr. Andre, even of any apprehenſion of danger. Dr. Thornton ſent me lately a caſe with eruptions, produced by matter which I originally took from a cow. According, then, to experience, we draw theſe concluſions:—1. That in certain conſtitutions, or under the circumſtances of certain co-operating agents, the vaccine poiſon produces a diſeaſe reſembling the ſmall-pox; and of courſe the puſtule in the inoculated part is very different from that of the vaccine pock ordinarily occurring, and the eruptions reſemble very much, if not exactly, ſome varieties of the ſmall-pox. 2. That in ſome inſtances theſe eruptions have occurred although the inoculated part exhibited the genuine vaccine puſtule. 3. That the matter of ſuch eruptive caſes, whether taken from the inoculated part, or from other parts, produces univerſally, or at leaſt generally, ſimilar eruptive caſes, and has not, I believe, been ſeen to go back, by paſſing through different conſtitutions, to the ſtate in which it produces what is called the genuine vaccine diſeaſe. 4. That eruptions, of a different appearance from variolous ones, ſometimes occur in the true cow-pock.

Now, whether the vaccine poiſon, when it produces theſe caſes reſembling the ſmall-pox, has really become, by compoſition or decompoſition, variolous matter, is undetermined.

If

If this fhould be found to be the cafe by future experiments, ftill we muft confider the two poifons as of diftinctly different fpecies, on account of the different characters of the puftule in the fmall-pox and cow-pox; although, as juft faid, by the combination of fome other fubftance with the cow-pox poifon, or by the feparation of fome one of the conftituent ingredients of this poifon, the variolous poifon may be produced. To illuftrate this theory let it be confidered, that magnefia and fulphate of magnefia are different fpecies of fubftances; although they agree in fome of their principal effects on the human conftitution, and in other properties; but, by the union of magnefia with fulphuric acid, it becomes fulphate of magnefia. Or the illuftration may be given converfely. As, then, we have diftinct denominations for thefe two fubftances, fo we ought to have them for the two poifons, and the two different difeafed ftates they produce, namely, the cow-pox and the fmall-pock. Accordingly, Dr. Odier, of Geneva, whofe powers as a Dialectician, and whofe acutenefs as a Philologer, I can atteft from the period of his academical ftudies, has "baptifed" (to ufe the language of Dr. De Carro,) the new difeafe *La Vaccine*, or *Vaccina*, rejecting the abfurd name of the Englifh, *variolæ vaccinæ*.

But, to return to the immediate queftion under difcuffion, granting that eruptions are liable to be produced by the inoculation of the cow-pock, what difference ought this accident to make in our eftimate of the value of the new practice, from the eftimate on the fuppofition that no fuch eruptions would occur? I apprehend the value is hereby depreciated, but not to fuch a degree as to create any reafonable apprehenfions of the failure of the Vaccine Inoculation in fuperfeding and extinguifhing the fmall-pox. Becaufe,

1. If the precaution be taken of avoiding the inoculation with matter from eruptive cafes, as far as I have feen, not above one cafe, with variolous-like eruptions, will be produced among 200 inftances of inoculated cow-pox.

2. Thefe eruptive cafes are, as far as I have obferved, like the ordinary kinds of inoculated fmall-pox.

3. I have feen no disfigurations of the fkin from this variety of cow-pox; but I think it juft to acknowledge that, from

from the experience I have had, no practitioner can anſwer for ſuch caſes not occurring in any inſtance, and as danger is always in proportion to the number of eruptions, which number no one can pretend to limit, it is evident that the chance of life during this diſeaſe is leſſened, although but a very little. Provided this ſtatement be made to the patient, it does not appear to me that the fact of the liability to eruptions ought to impede the progreſs of the Vaccine Inoculation; but if, on the contrary, the patient is aſſured that ſuch eruptions will not occur, there is good ground for the practice falling into diſcredit, or at leaſt for many perſons, with reaſon, being diſcontented.

After this repreſentation of an unfavourable part of the hiſtory of the cow-pock, it is conſolatory to be able to counterpoiſe it with ſome new facts, which, like that of the eruptions, have been diſcovered in the courſe of further experience. It now appears that a perſon who has had the ſmall-pox, is not ſuſceptible of the cow-pox by inoculation *; nor is a perſon ſuſceptible of the conſtitutional affection from the cow-pox poiſon more than once. On the whole, then, we have gained perhaps as much as we have loſt ſince the publication of the original account: and unleſs ſome new adverſe facts ſhall be diſcovered, and confiding that the public will adopt a practice which is ſo manifeſtly to their intereſt, the change effected in the practice of medicine will be ſo eminently memorable, *that the introduction of the Vaccine Inoculation muſt become an epoch in the hiſtory of phyſic.*

II. *Obſervations reſpecting the Earthquake which took place in Peru in the year* 1797. *By* M. CAVANILLES †.

THERE are reckoned to be more than ſixteen volcanoes in the kingdom of Quito which are in a continual ſtate of eruption, and which throw up thick vapours, often mixed

* The caſes of milkers with chopt hands being repeatedly affected by the cow-pox poiſon in the *caſual way*, whether they had undergone the ſmall-pox or not, probably occaſioned the error of the concluſions here alluded to.

† From the *Journal de Phyſique*, Fructidor an. 7.

with

with flames, either from their craters or through their lateral fiſſures. In the midſt of the moſt profound calm there is frequently heard a dreadful bellowing noiſe, the forerunner of earthquakes, to which this part of the world is often expoſed. After the year 1791, this noiſe was frequently heard in the neighbourhood of the mountain of Tunguragua. Antonio Pineda and Née, the two naturaliſts employed in the expedition round the world, when examining the declivity of this volcano, the lava of which had been hardened more by the internal fire than by the ardour of the ſun, were ſtruck with terror by the horrible ſound which they heard, and the heat which they experienced. Pineda, that valuable member of ſociety, whoſe premature death is ſtill deplored by the friends of ſcience, foretold that a terrible eruption was preparing in the mountain of Tunguragua; and his conjectures were confirmed by the event. On the 4th of February 1797, at three quarters paſt ſeven in the morning, the ſummit of the volcano was more free from vapours than uſual; the interior part of the mountain was agitated by frequent ſhocks, and the adjacent chains burſt in ſuch a manner, that in the ſpace of four minutes an immenſe tract of country was convulſed by an undulating movement. Never did hiſtory relate the effects of an earthquake ſo extraordinary, and never did any phenomenon of Nature produce more misfortunes, or deſtroy a greater number of human beings. A number of towns and villages were deſtroyed in a moment: ſome of them, ſuch as Riobamba, Quero, Pelileo, Patate, Pillaro, were buried under the ruins of the neighbouring mountains; and others in the juriſdictions of Harnbata, Latacunga, Guaranda, Riobamba, and Alauſi, were entirely overthrown. Some ſuſtained prodigious loſs by the gulphs which were formed, and by the reflux of rivers intercepted in their courſe by mounds of earth; and others, though in part ſaved, were in ſuch a ſhattered ſtate as to threaten their total ruin. The number of perſons who periſhed during the firſt and ſucceeding ſhocks are eſtimated at 16,000. At ten o'clock in the morning, and four in the afternoon, the ſame day, (February 4,) after a dreadful noiſe, the earth was again agitated with great violence, and it did not ceaſe to ſhake,

though

though faintly, for the whole month of February and March; but, at three quarters paſt two in the morning of the 5th of April, the villages already ruined were again expoſed to ſuch violent ſhocks as would have been ſufficient to deſtroy them. This extraordinary phenomenon was felt throughout the extent of 140 leagues from eaſt to weſt, from the ſea as far as the river Napo; and without doubt farther, for we are little acquainted with theſe diſtricts which are inhabited by the ſavages. The diſtance north-eaſt and ſouth-weſt between Popajan and Piura, is reckoned to be 170 leagues; but in the centre of that diſtrict, 1 degree 16·6 from theſe places, is ſituated the part totally deſtroyed, and which comprehends forty leagues from north to ſouth between Guarandam and Machache, and twenty leagues from eaſt to weſt. But, as if an earthquake alone had not been ſufficient to ruin this fertile and populous country, another misfortune, hitherto unknown, was added. The earth opened, and formed immenſe gulphs; the ſummits of the mountains tumbled down into the valleys, and from the fiſſures in their ſides there iſſued an immenſe quantity of fetid water, which in a little time filled up valleys a thouſand feet in depth and ſix hundred in breadth. It covered the villages, buildings, and inhabitants; choaked up the ſources of the pureſt ſprings, and, being condenſed by deſiccation, in the courſe of a few days, into an earthy and hard paſte, it intercepted the courſe of rivers, made them flow backwards for the ſpace of eighty-ſeven days, and converted whole diſtricts of dry land into lakes. Very extraordinary phenomena, which will doubtleſs be one day mentioned in hiſtory, occurred during theſe earthquakes; I ſhall, however, content myſelf with mentioning only two of them. At the ſame moment that the earth ſhook, the lake of Quirotoa, near the village of Inſiloc, in the juriſdiction of Lacatunga, took fire, and the vapour which roſe from it ſuffocated the cattle and flocks that were feeding in the neighbourhood. Near the village of Pelileo, a large mountain named Moya, which was overturned in an inſtant, threw out a prodigious ſtream of the before-mentioned thick fetid matter, which deſtroyed and covered the miſerable remains of that city. Naturaliſts will

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one day find, in thefe ravaged countries, objects worthy of their refearches. Fragments of the minerals and earths of Tunguragua are about to be tranfported to Spain: but it is not in fuch fragments that we ought to fearch for the caufe of thefe furprifing phenomena; we muft vifit the country itfelf, where this conflict of the elements took place, and where the ruins it occafioned are ftill to be feen *.

III. *Experiments to determine the Quantity of Tanning Principle and Gallic Acid contained in the Bark of various Trees.* *By* GEORGE BIGGIN, *Efq.*†

THE bark of trees contains the aftringent principle called gallic acid, and alfo that principle which has a peculiar affinity to the matter of fkin, and which, from the ufe to which it is applied, is called the tanning principle. But, in the prefent mode of tanning, bark is applied in *mafs* to the fkins; confequently, *both* principles are applied. It remains for examination, whether both principles are ufeful in the procefs of tanning; for, if they are not both ufeful, probably *one* is detrimental.

To a nobleman, whofe zeal on every occafion by which the fciences or arts may receive illuftration or improvement, is eminently confpicuous, and to whofe public energy, as well as private friendfhip, I feel myfelf much indebted, to his Grace the Duke of Bedford, I owe the means of profecuting fome experiments on this fubject. His Grace, by collecting a variety of barks, at Woburn, gave me an opportunity of making fome experiments to afcertain the quantity of tanning principle and gallic acid each bark contained. For that purpofe I made ufe of the following methods, according to the principles laid down by M. Seguin:—

By diffolving an ounce of common glue in two pounds of boiling water, I procured a mucilaginous liquor, which, as it contains the matter of fkin in folution, is a teft for the

* The volcano of Tunguragua occafioned an earthquake in 1557.

† From the *Philofophical Tranfactions of the Royal Society of London.*

 tanning

tanning principle. By a ſaturated ſolution of ſulphat of iron, I obtained a teſt for the gallic acid.

I then took one pound of the bark I meant to try, ground as for the uſe of tanners, and divided it into five parts, each part being put into an earthen veſſel. To one part of this bark I added two pounds of water, and infuſed them for one hour. Thus I procured an infuſion of bark, which I poured on the ſecond part of the bark, and this ſtrengthened infuſion again on the third part, and ſo on to the fifth. But, as a certain portion of the infuſion will remain attached to the *wood* of the bark after the infuſion is poured or drawn off, I added a third pound of water to the firſt part, and then followed up the infuſion on the ſeveral parts till the three pounds of water, or ſo much of them as could be ſeparated from the bark, were united in the fifth veſſel; from which I generally obtained about one pint of ſtrong infuſion of bark *.

To a certain quantity of this infuſion, I added a given meaſure of the ſolution of glue, which formed an immediate precipitate, that may be ſeparated from the infuſion by filtering paper. When dried, it is a ſubſtance formed by the chemical union of the matter of ſkin with the tanning principle, and is, in fact, a powder of leather. By ſaturating the infuſion with the ſolution of glue, the whole of the tanning principle may be ſeparated by precipitation.

For the Gallic Acid.

To the pound of bark left in the earthen veſſels, and already deprived of its tanning principle by theſe *quick* infuſions, I added a *given* quantity of water, to procure a ſtrong infuſion of the gallic acid, which requires a longer time, (ſay forty-eight hours.) This infuſion, when obtained pure †, affords little ſigns of the preſence of the tanning principle,

* The ſpecific gravity of this infuſion was aſcertained by an hydrometer whoſe gradations are inverſe to thoſe of a ſpirit hydrometer.

† It is hardly poſſible, from the intimate connection of the two principles, to ſeparate them entirely by infuſion: in the infuſion of tanning principle, there will always exiſt a little gallic acid; and, in an infuſion of gallic acid, a little tanning principle will commonly be preſent, unleſs the infuſion of gallic acid is very weak, and procured by a third or fourth watering.

when

when tried by the teſt of the ſolution of glue; but, with the ſolution of ſulphat of iron, it gives a ſtrong black colour, (the common black dye,) which differs in denſity according to the quality of the bark: this may be further proved, by boiling a ſkain of worſted in the dye, by which the gradations of colour will be very perceptibly demonſtrated.

Having thus obtained a point of compariſon; by making a ſimilar infuſion, under ſimilar circumſtances, of any bark, or vegetable ſubſtance, and paying ſtrict attention to the ſpecific gravity of the infuſion, the quantity of precipitate of leather, and the denſity of colour produced by given quantities of one or the other teſt, the reſult will be, a comparative ſtatement of the reſpective powers of any bark, or vegetable ſubſtance. This comparative ſtatement I conceive to be ſufficient for all commercial purpoſes.

As oak bark is the uſual ſubſtance employed in the trade of tanning, if a quantity of tanning principle is found to be contained in any other bark or vegetable, the commercial utility of that bark or vegetable may be determined, by comparing its quantity of tanning principle and price with thoſe of oak bark.

For an accurate chemical analyſis, I have tried a variety of acids, and ſimple and compound affinities; and, having purſued the above experiments at the ſame time that I was employed on ſome in dyeing, I found the muriat of tin (the method of uſing which is deſcribed by Mr. Prouſt in the *Annales de Chimie*,) very convenient. A ſolution of it being added to the infuſion of bark, forms a precipitate with the tanning principle, leaving the gallic acid ſuſpended: the precipitate is of a fawn colour, and is compoſed of tanning principle and oxydated tin.

By theſe means I have been enabled to form a comparative ſcale of barks; which, however, I do not produce as accurate. Oak bark, in its preſent ſtate, as procured for commercial purpoſes, differs very much in quality, from accidental circumſtances: the ſeaſon of the year in which it is collected occaſions a ſtill more important difference, conſequently the ſcale now produced muſt be very imperfect; but I am of opinion, that, by the purſuits of ſcientific men,

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who may be inclined to inveſtigate this ſubject more fully, a very accurate ſcale may hereafter be formed.

In the following ſcale, I have taken Sumach as the moſt powerful in the comparative ſtatement; leaving, however, a few degrees for a *ſuppoſed maximum of tanning principle*, which I reckon twenty.

SCALE OF BARKS.

Bark of	Gallic acid, by colour.	Tanning principle, by hydrometer.	Tanning principle, (in grains,) from half a pint of infuſion and an ounce of ſolution of glue.
Elm * - - -	7	2,1	28
Oak, cut in winter	8	2,1	30
Horſe cheſtnut -	6	2,2	30
Beech - - -	7	2,4	31
Willow (boughs)	8	2,4	31
Elder - - -	4	3,0	41
Plum-tree - -	8	4,0	58
Willow (trunk) -	9	4,0	52
Sycamore - -	6	4,1	53
Birch - - -	4	4,1	54
Cherry-tree - -	8	4,2	59
Sallow - - -	8	4,6	59
Mountain aſh -	8	4,7	60
Poplar - - - -	8	6,0	76
Hazel - - -	9	6,3	79
Aſh - - - -	10	6,6	82
Spaniſh cheſtnut -	10	9,0	98
Smooth oak - -	10	9,2	104
Oak, cut in ſpring	10	9,6	108
Huntingdon or Leiceſter willow -	10	10,1	109
Sumach - - -	14	16,2	158

It is to be obſerved, that the barks do not keep any reſpective proportion in the quantity of gallic acid and tanning principle contained in each; which is an evidence of the

* The infuſion of elm was ſo loaded with mucilage that it was with difficulty I could ſeparate the tanning principle, or try the ſpecific gravity.

diſtinctneſs

diſtinctneſs of principle, and may perhaps open a new field for ſaving oak bark in dyeing, as the willows, ſallow, aſh, and others, produce a very fine black. It is alſo worthy of obſervation, that the quantities of gallic acid and tanning principle do not differ in *equal* proportions between the winter and ſpring felled oaks. This fact may lead to the diſcrimination of the proper time for cutting; which is, probably, when the ſap has completely filled and dilated that part of the vegetable intended for uſe. This will make a difference in the ſeaſon of cutting oak, elm, and other trees, ſhrubs, &c. Leaves ſhould be taken when arrived at their full ſize, and then dried under cover; for, as the tanning principle is ſo ſoluble, and the ſubſtance that contains it ſo thin, (in a leaf,) the dew alone might diſſolve it.

Finally, as the gallic acid does not ſeem to combine with the matter of ſkin, and as its aſtringency will corrugate the ſurface, we may, I think, conclude, that its preſence in tanning is not only uſeleſs, but detrimental.

IV. *Extract of a Memoir on the Grecian Method of dyeing Cotton Yarn Red. By* C. Felix*.

THAT beautiful red dye given to cotton in the Ottoman empire, is known in Europe under the name of Turkey red, Levant red, or Adrianople red. As it is believed among us that this colour reſults chiefly from the proceſſes employed in the dyeing, I ſhall give an account of thoſe followed in the Grecian manufactories. It muſt, however, be obſerved, that in theſe manufactories the workmen dye at one time a maſs of ſkains weighing thirty-five occas; each occa being equal to about fifty ounces.

The firſt proceſs is that of cleaning the cotton, for which purpoſe three leys are employed; one of ſoda, another of aſhes, and a third of lime. The cotton is thrown into a tub, and moiſtened with the liquor of the three leys in equal quantities; it is then boiled in pure water, and waſhed in running water.

* From the *Annales de Chimie*, No. 92.

The

The second bath given to the cotton is composed of soda and sheep's dung dissolved in water. To facilitate the solution the soda and dung are pounded in a mortar. The proportions of these ingredients employed, are, one occa of dung, six of soda, and forty of water. When the ingredients are well mixed, the liquor expressed from them is strained, and being poured into a tub, six occas of olive oil are added to it, and the whole is well stirred till it becomes of a whitish colour, like milk. The cotton is then besprinkled with this water, and when the skains are thoroughly moistened, they are wrung, pressed, and exposed to dry. The same bath must be repeated three or four times, because it is this liquor which renders the cotton more or less fit for receiving the dye. Each bath is given with the same liquor, and ought to continue five or six hours. It is to be observed that the cotton, after each bath, must be dried without being washed, as it ought not to be rinsed till after the last bath. The cotton is then as white as if it had been bleached in the fields.

The bath of sheep's dung is not used in our manufactories; it is a practice peculiar to the Levant. It may be believed that the dung is of no utility for fixing the colours; but it is known that this substance contains a great quantity of volatile alkali, in a disengaged state, which has the property of giving a rosy hue to the red. It is therefore probable that it is to this ingredient that the red dyes of the Levant are indebted for their splendour and vivacity. This much, at any rate, is certain, that the Morocco leather of the Levant is prepared with dog's dung; because it has been found that this dung is proper for heightening the colour of the lack. The bath of dung is followed by the process of galling.

The galling is performed by immersing the cotton in a bath of warm water, in which five occas of pulverised gall-nuts have been boiled. This operation renders the cotton more fit for being saturated with the colour, and gives to the dye more body and strength. After the galling comes aluming, which is performed twice, with an interval of two days, and which consists in dipping the cotton into a bath of water in which five occas of alum have been infused, mixed with five occas of water alcalised by a ley of soda. The aluming must

muſt be performed with care, as it is this operation which makes the colouring particles combine beſt with the cotton, and which ſecures them in part from the deſtructive action of the air. When the ſecond aluming is finiſhed, the cotton is wrung; it is then preſſed, and put to ſoak in running water, after being incloſed in a bag of thin cloth.

The workmen then proceed to the dyeing.—To compoſe the colours they put in a kettle five occas of water and thirty-five occas of a root which the Greeks call *ali-zari*, or painting colour, and which in Europe is known under the name of *madder*. The madder, after being pulveriſed, is moiſtened with one occa of ox or ſheep's blood. The blood ſtrengthens the colour, and the doſe is increaſed or leſſened according to the ſhade of colour required. An equal heat is maintained below the kettle, but not too violent; and when the liquor ferments, and begins to grow warm, the ſkains are then gradually immerſed before the liquor becomes too hot. They are then tied with packthread to ſmall rods, placed croſſwiſe above the kettle for that purpoſe, and when the liquor boils well, and in an uniform manner, the rods from which the ſkains were ſuſpended are removed, and the cotton is ſuffered to fall into the kettle, where it muſt remain till two-thirds of the water is evaporated. When one-third only of the liquor remains, the cotton is taken out and waſhed in pure water.

The dye is afterwards brought to perfection by means of a bath alcaliſed with ſoda. This manipulation is the moſt difficult and the moſt delicate of the whole, becauſe it is that which gives the colour its tone. The cotton is thrown into this new bath, and made to boil over a ſteady fire till the colour aſſumes the required tint. The whole art conſiſts in catching the proper degree: a careful workman, therefore, muſt watch with the utmoſt attention for the moment when it is neceſſary to take out the cotton, and he will rather burn his hand than miſs that opportunity.

It appears that this bath, which the Greeks think of ſo much importance, might be ſupplied by a ley of ſoap; and it is probable that ſaponaceous water would give the colour more brightneſs and purity.

When

When the colour is too weak, the Levantines know how to ſtrengthen it by increaſing the doſe of the colouring ſubſtances; and when they wiſh to give it brightneſs and ſplendour, they employ different roots of the country, and, in particular, one named *ſaſſari*, ſpecimens of which I have ſent to France.

The ali-zari, which is the principal colouring matter employed in the Greek dye-houſes, is collected in Natolia, and is brought to Greece from Smyrna: ſome of it comes alſo from Cyprus and Meſopotamia. The ſuperiority of this Levantine plant to the European madder is acknowledged by all thoſe acquainted with the art of dyeing, and may ariſe from two cauſes; the manner in which it is cultivated, and the method employed for its deſiccation.

As the ali-zari appears to be of a weaker conſtitution than the common madder: as its branches are more delicate, its leaves ſmoother and tenderer, and its ſtem more fragile, it is ſupported by ſticks, as peaſe are among us. A well nouriſhed ſtem acquires in this manner more conſiſtence, and ſends forth more roots. It is to be obſerved, that it is the woody part of the roots which affords the greateſt quantity of colouring particles. The ali-zari is not collected till the fifth or ſixth year, that is to ſay, when it has acquired its full ſtrength; while in France and Zealand, where land is of more value than in the Levant, the inhabitants wiſh to reap too ſoon, and collect the madder before it comes to complete maturity.

The method employed in the deſiccation contributes alſo to improve the quality of the ali-zari. The Levantines dry it in the open air; and this operation is eaſy in a country where great dryneſs prevails in the atmoſphere, while, in our damp climates, we are obliged to dry the madder by ſtoves. Hence it happens that the ſmoke, which mixes itſelf with the cold air, and penetrates the roots, impregnates them with fuliginous particles, which alter the colouring ſubſtance; an accident which does not take place when the madder is dried without the aſſiſtance of fire. It is, however, poſſible, that the ali-zari and madder may never produce the ſame tone of colour, notwithſtanding all the care that may be employed

 in

in the culture and drying of the latter; becauſe there may be as much difference between theſe two plants, ſo like to each other, as between the goats of France and thoſe of Angora.

It is probable, alſo, that the ſuperiority of the ali-zari over madder may ariſe only from its ſtem being tenderer, and by its conſequently having a greater diſpoſition to transform itſelf into ſucculent roots. In that caſe, ſeveral of our indigenous plants of the family of the *rubiæ* might be ſubſtituted in its place with advantage; ſuch as the *gallium luteum* and *gallium flore albo*, found in abundance on the hills of Poitou, and the madder, which grows on both ſides of the Alps, and which has been diſtinguiſhed by the name of *rubia lævis Taurinenſium*. All theſe plants give a red as beautiful and as pure as the beſt madder; but I do not know whether the quantity of their colouring matter is equal to their beautiful quality.

The chief manufactories for dyeing ſpun cotton red, eſtabliſhed in Greece, are in Theſſaly. There are ſome at Baba, Rapſani, Tournavos, Lariſſa, Pharſalia, and in all the villages ſituated on the ſides of Oſſa and Pelion. Theſe two mountains may be conſidered as the alembics that diſtil the eternal vapours with which Olympus is crowned, and which diſtribute them throughout the beautiful valleys ſituated around them. Of theſe valleys, that of Tempe has at all times been diſtinguiſhed by the beauty of its ſhady groves and of its ſtreams. Theſe ſtreams, on account of their limpidneſs, are very proper for dyeing, and ſupply water to a great number of manufactories, the moſt celebrated of which are thoſe of Ambelakia.

Ambelakia, on account of the activity which prevails in it, has a greater reſemblance to a town of Holland than a village of Turkey. This village, by its induſtry, communicates life and activity to all the neighbouring country, and gives birth to an immenſe trade, which connects Germany with Greece in a thouſand ways. Its population, which has been tripled within theſe fifteen years, amounts at preſent to 4000, and all theſe people exiſt by dyeing. None of thoſe vices or cares produced by idleneſs are known here. The hearts of

 the

the inhabitants are pure, and their countenances unclouded. Servitude, which degrades the countries watered by the Peneus, has not yet afcended to thefe hills: no Turk can refide or live among thefe people; and they govern themfelves, like their anceftors, by their *protoyeros* and their own magiftrates. Twice have the favage muffulmans of Lariffa, envious of their eafe and happinefs, attempted to fcale their mountains in order to plunder their houfes; and twice have they been repulfed by hands which fuddenly quitted the fhuttle to affume the mufket.

All hands, and even thofe of the children, are employed in the dye-houfes of Ambelakia; and while the men dye the cotton, the women are fpinning and preparing it. The ufe of wheels is not known in this part of Greece; all the cotton is fpun on a diftaff: the thread, indeed, is certainly not fo round or equal, but it is fofter, more filky, and more tenacious; it is lefs apt to break, and lafts longer; it is alfo more eafily whitened, and more proper for being dyed. It is a pleafing fpectacle to fee the women of Ambelakia, each fpinning from a diftaff, and fitting converfing together on the threfhold of their doors; but as foon as a ftranger appears, they inftantly retire and conceal themfelves in their houfes, manifefting, like Galatea, in their precipitate retreat, a defire of flying and of fhewing themfelves:—

Et fugit ad falices, et fe cupit ante videri.

The eye can only have a curfory view of the fhape of fome of thefe women; but it ftill obferves, with furprife, thofe flender and graceful forms of the ancient Grecian females, which ferved as models to the moft beautiful ftatues in the world.

V. *An Account of the Wild Horfes in Spanifh America.* *By* D. Felix Azara *.

THESE animals were originally carried from Spain by the firft conquerors, and are of the Andalufian breed. They chiefly

* From a work, not yet publifhed, on the natural hiftory of Paraguay, by Don Felix Azara, brother of the late Spanifh ambaffador at Paris. This extract is tranflated from the *Decade Philofophique*, No. 9, year 8.

chiefly frequent the fouthern part of the river de la Plata, as far as Rio Negro, the country of the Patagonians, &c. The wild horfes of all thefe countries live in numerous herds, fome of which, it is faid, confift of ten thoufand. As foon as they perceive domeftic horfes in the fields, they run towards them on a full gallop, pafs through the middle of them, or near them, carefs them, and invite them with a kind of grave and prolonged neighing. The domeftic horfes are foon feduced, unite themfelves to the independent herd, and depart along with them. It happens not unfrequently that travellers are ftopped on the road by the effect of this defertion. To prevent it, they halt as foon as they perceive thefe wanderers, watch their own horfes, and endeavour to frighten away the others. In fuch cafes the wild horfes follow a certain kind of tactics: fome are detached before, and the reft advance in a clofe column, which nothing can interrupt. If they are fo alarmed as to be obliged to retire, they change their direction, but without fuffering themfelves to be difperfed: fometimes they make a great number of turns around thofe which they wifh to feduce, in order to frighten them; at other times they retire after making one turn. Thefe manœuvres are not employed during the night, for the wild horfes then make no attempts. The author is ignorant whether any thing of the fame kind takes place between one herd of wild horfes and another for the purpofe of recruiting their number. He afferts that Buffon is miftaken in faying that thefe wild horfes have more ftrength and fleetnefs than the domeftic horfes of the country, and that they do not differ from the latter either in height or fhape: but he indeed obferves, that no comparifon can be made between the independent and domeftic ftate of thefe animals, as in that country both ftates are almoft fimilar.

Thofe who poffefs *eftancias*, or paftures for keeping domeftic horfes, place in them a certain number of mares, which are never broke or mounted. They remain during their whole lives in a ftate of perfect liberty; and for every thirty or forty mares there is a ftallion, which enjoys the fame independence. They are counted once or twice a week, in order that they may not ftray from the habitation. But

too little care is employed in the choice of the ſtallions; and this, in all probability, is one of the principal cauſes why theſe horſes, though left at full liberty, are neither ſo beautiful nor ſo good as thoſe of Andaluſia, from which they are deſcended. The induſtry of man improves the breed of thoſe animals which he appropriates to his own uſe, and, by bringing their ſhape and organs to perfection, indemnifies them for that liberty of which he deprives them. Each ſtallion takes poſſeſſion of a ſmall troop of mares, which he keeps collected by preſſing them with his cheſt, and by biting them if they do not obey with ſufficient docility: the mares, on the other hand, remain attached to their ſultans. If two ſtallions fight, the mares do not abandon the conquered for the conqueror, unleſs the former has ſhewn among them a deficiency of vigour.

The fillies remain with their mothers. When it is time to break the colts they are cut, for no perſon mounts a horſe until that operation has been performed. After caſtration, a halter is put upon the animal intended to be broke; he is tied to a ſtake; a ſaddle is placed upon his back, and well girded, but without crupper or breaſt-leather, and a thong is tied round his under-lip, to which is faſtened, on each ſide, a rein, in order to govern him. The horſeman then mounts with large ſpurs, and rides out into the fields. At firſt the young horſe capers and jumps until he is quite fatigued; after which he is brought back to the ſtake. This exerciſe is repeated ſeveral times in the courſe of the ſame day, and is renewed after the interval of ſeveral days until the courſer capers no more. He is then employed as a broke horſe, but with a halter only; a bridle is not put upon him till a year after, at which time he quits the name of *rodomont* to aſſume that of horſe.

As ſoon as the horſes are cut, they are ſeparated from the mares, and put among the animals of the horſe ſpecies uſed for ſervice, which receive no other ſhelter or food than what they find in the fields. They are accuſtomed to live in one canton, which they never quit. Each unites himſelf to a companion, and with ſuch intimacy that inſtances have been known of ſome of them, after running away, having returned

returned more than ſixty leagues to rejoin their old friend. Theſe friends know each other by their neighing, their ſmell, and the noiſe of their pace.

When the proprietors are deſirous to prevent a numerous troop of domeſtic horſes from ſeparating, they place among them a young mare with a ſmall bell, and which is then called the godmother. They all follow her, and they all know and ſeek for each other as members of the ſame ſociety. The ſame effect may be produced by attaching the bell to one of the horſes of the troop.

When the inhabitants have need of horſes, a man on horſeback, bearing a lance, proceeds towards a troop of theſe animals, and drives them into an incloſure formed of palliſades. A horſeman then enters it, and, when he is within reach, entangles the horſe which he wiſhes to catch with a kind of rope; for, though theſe horſes are fit to be mounted, and ſaid to be docile, they will not ſuffer themſelves to be touched with the hand.

From what has been ſaid, it may be readily ſeen that there is really very little difference between the habits of theſe wild horſes and of thoſe which live in a ſtate of domeſticity. It needs excite no wonder, therefore, that there ſhould be very little in regard to their form, ſize, and qualities.

When the inhabitants wiſh to convert ſome of theſe wild horſes into domeſtic ones, people mounted on horſeback proceed towards a troop of the former; and when they approach them they throw ſome of the ropes already mentioned around their legs, ſo that, being prevented from running, they have time to ſecure them. They are then tied to a ſtake, or a tree, not by the four legs, as ſome have ſaid, but with a ſimple halter made of leather. They are left two or three days without food or drink, are afterwards cut, and are then broke in the ſame manner as the domeſtic horſes. The horſe behaves then as if he had never been wild; but it is not true that they loſe all deſire of recovering their liberty. They readily unite with a troop of wild horſes; and how can it be otherwiſe, ſince thoſe even which have been reared in a ſtate of domeſticity have no repugnance to join them?

The proprietors of the *eſtancias*, or paſtures deſtined for

 thoſe

thofe horfes which have been tamed, endeavour not only to frighten away the wild horfes, but even to exterminate them. With that view they beat the woods in queft of them, drive them, if poffible, into ravines, and kill them by means of lances.

The Pampas eat their flefh, and particularly that of the fillies, colts, and mares; but they fometimes kill a very fat ftallion to make a fire with his greafe and bones, as in the country of Pampa wood is extremely fcarce.

The Spanifh author often contradicts Buffon, not only in regard to local obfervations, which is not aftonifhing, but alfo in regard to general ideas, fuch as the influence of climate, &c. D. Felix Azara refutes the affertion of the French naturalift, who afcribes more ftrength and fleetnefs to the wild than to the domeftic horfes of thefe countries. He even afferts, that he has not been correct in faying that each wandering troop fubmit, by common confent, to a chief, which ferves as a guide; which regulates and directs their movements; forms them in the order of battle, by files, companies, fquadrons, &c. The truth, according to the author, is, that each ftallion appropriates to himfelf as many mares as he can, which he takes care of, and keeps collected; that he combats any other ftallion which attempts to deprive him of them; and that each wandering troop confifts, therefore, of a number of fmall diftinct bodies, which fometimes unite into one.

In the great number of wild troops which the author faw, he never obferved any other prevailing colours than bay, dark brown, and jet black. If it fometimes happens that a pied or dirty grey coloured individual is feen, or one of any other colour, it may with certainty be concluded that it is a domeftic horfe which has deferted. According to the author, there are ninety bay for one dark brown horfe; and black horfes are fo uncommon, that one of them is fcarcely feen in two thoufand. He thence infers, that thefe three colours, bay, dark brown, and jet black, are a primitive mark, which diftinguifhes, at leaft in part, the horfes which recover their liberty; that the firft horfe and mare which exifted had one of thefe three colours, and moft probably the bay, fince it appears

appears that, among the wild horfes, the black is becoming extinct, and that this will be the cafe alfo with the brown; that, taking the colour as an index, we might fay, that the beft breed of horfes is the bay, then the brown, and next the black; all the other colours being inferior, as they are the refult of more diftant degradations from the primitive horfe, which muft have been the moft perfect. Experience feems, in fome meafure, to confirm thefe conjectures; for, except in a few cafes, which are of little confequence, the bays are the moft efteemed, and the browns hold the next rank. He obferves, that in France a prejudice is entertained againft the laft-mentioned colour, which he thinks unreafonable, and which in his opinion feems to fhew that the French, in this refpect, have not fo much difcernment as the Spaniards. Thefe obfervations, and the inferences which the author draws from them, feem to weaken the confidence which might be placed in what has been faid by Buffon, on the authority of Herodotus, Leo Africanus, and Marco Polo, of wild white horfes faid to have exifted in Arabia and Numidia. We know how fufpicious the teftimony of the ancients is in regard to natural hiftory, and that the authority of Buffon himfelf has little weight when he gives teftimonies inftead of obferved facts.

VI. *An Account of the Pearl Fifhery in the Gulph of Manar, in March and April* 1797. *By* HENRY J. LE BECK, *Efq.**

FROM the accounts of the former pearl fifheries at Ceylon, it will be found, that none have ever been fo productive as this year's. It was generally fuppofed that the renter would be infallibly ruined, as the fum he paid for the prefent fifhery was thought exorbitant when compared with what had been formerly given: but this conjecture in the event appeared ill-founded, as it proved extremely profitable and lucrative.

The farmer this time was a Tamul merchant, who, for

* From the *Afiatic Refearches*, Vol. V.

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the privilege of fishing with more than the usual number of donies or boats, paid between two and three hundred thousand Porto-novo pagodas; a sum nearly double the usual rent.

These boats he farmed out again to individuals in the best manner he could, but for want of a sufficient number of divers some of them could not be employed.

The fishing, which commonly began about the middle of February, if wind and weather allowed, was this year, for various reasons, delayed till the end of the month; yet, so favourable was the weather, that the renter was able to take advantage of the permission granted by the agreement, to fish a little longer than the usual period of thirty days.

The fishery cannot well be continued after the setting-in of the southern monsoon, which usually happens about the 15th of April, as, after that time, the boats would not be able to reach the pearl banks, and the water being then so troubled by heavy seas, diving would be impracticable; in addition to which, the sea-weed, a species of *fucus*, driven in by the southerly wind, and which spreads to a considerable distance from the shore, would be an impediment.

Many of the divers, being Roman Catholics, leave the fishery on Sundays to attend divine service in their church at Aripoo; but if either a Mahomedan or Hindoo festival happens during the fishing days, or if it is interrupted by stormy weather, or any other accident, this lost time is made up by obliging the Catholics to work on Sundays.

The fear of sharks, as we shall see hereafter, is also another cause of interruption. These, amongst some others, are the reasons that, out of two months, (from February till April,) seldom more than thirty days can be employed in the fishery.

As this time would be insufficient to fish all the banks, (each of which has its appropriate name, both in Dutch and Tamul,) it is carried on for three or four successive years, and a new contract annually made, till the whole banks have been fished; after which they are left to recover.

The length of time required for this purpose, or from one general fishing to another, has not yet been exactly determined; it was therefore a practice to depute some persons to

visit

viſit the banks annually, and to give their opinion, whether a fiſhery might be undertaken with any degree of ſucceſs * ?

From various accounts, which I have collected from good authority, and the experience of thoſe who aſſiſted at ſuch examinations, I conjecture, that every ſeven years ſuch a general fiſhery could be attempted with advantage, as this interval ſeems ſufficient for the pearl ſhells to attain their growth: I am alſo confirmed in this opinion by a report, made by a Dutch governor at Jafnas, of all the fiſheries that have been undertaken at Ceylon ſince 1722; a tranſlation of which is to be found in Wolfe's Travels into Ceylon. But the ruinous condition in which the divers leave the pearl banks at each fiſhery, by attending only to the profit of individuals, and not to that of the public, is one great cauſe that it requires twice the above-mentioned ſpace of time, and ſometimes longer, for rendering the fiſhing productive. They do not pay the leaſt attention to ſpare the young and immature ſhells that contain no pearl; heaps of them are ſeen thrown out of the boats as uſeleſs on the beach between Manâr † and Aripoo: if theſe had been ſuffered to remain in their native beds, they would, no doubt, have produced many fine pearls. It might therefore be adviſable to oblige the boat-people to throw them into the ſea again before the boats leave the bank. If this circumſpection, in ſparing the ſmall pearl ſhells to perpetuate the breed, was always obſerved, ſucceeding fiſheries might be expected ſooner, and with ſtill greater ſucceſs: but the neglect of this ſimple precaution will, I fear, be attended with ſimilar fatal conſequences here, as have already happened to the pearl banks on the coaſt of Perſia, South America, and Sweden, where the fiſheries are by no means ſo profitable at preſent as they were formerly.

Another cauſe of the deſtruction of numbers of both old and young pearl ſhells is the anchoring of ſo many boats

* A gentleman, who aſſiſted at one of the laſt viſits, being an engineer, drew a chart of the banks, by which their ſituation and ſize are now better known than formerly.

† Manara, properly Manâr, is a Tamul word, and ſignifies a ſandy river, from the ſhallowneſs of the ſea at that place.

 on

on the banks; almoſt all of them uſed differently formed, clumſy, heavy, wooden anchors, large ſtones, &c. &c. If this evil cannot be entirely prevented, it might, at leaſt, be greatly leſſened, by obliging them all to uſe anchors of a particular ſort, and leſs deſtructive.

This ſeaſon the Seewel Bank only was fiſhed, which lies above twenty miles to the weſtward of Aripoo, oppoſite to the freſh water rivers of Mooſalee, Medragam, and Pompa-ripoo. It has been obſerved, that the pearls on the north-weſt part of this bank, which conſiſts of rock, are of a clearer water than thoſe found on the ſouth-eaſt, neareſt the ſhore, growing on corals and ſand.

Condatchey is ſituated in a bay, forming nearly a half moon, and is a waſte, ſandy diſtrict, with ſome miſerable huts built on it. The water is bad and brackiſh, and the ſoil produces only a few, widely ſcattered, ſtunted trees and buſhes. Thoſe perſons who remain here during the fiſhery are obliged to get their water for drinking from Aripoo, a village with a ſmall old fort, lying about four miles to the ſouthward. Tigers, porcupines, wild hogs, pangolines, or the *Ceylon armadillos*, are, amongſt other quadrupeds, here common. Of amphibia, there are tortoiſes, eſpecially the *teſtudo geometrica*, and various kinds of ſnakes. A concho-logiſt meets here with a large field for his enquiries. The preſents which I made to the people employed in the fiſhery, to encourage them to collect all ſorts of ſhells which the divers bring on ſhore, produced but little effect; as they were too much taken up in ſearching after the mother-of-pearl ſhells to pay attention to any other object. However, my endeavours were not entirely uſeleſs; I will ſpecify here a few of the number I collected during my ſtay: different kinds of *pectines* *, *palium porphyreum*, *ſolen radiatus* †, *Venus caſtrenſis Linn.* ‡ *aſtrea hyotis* §, *oſtr. Forſkolii*, *oſtr. Malleus* ||, *mytilus hirundo Linn.* ¶, *ſpondilus crocius*, *pholas puſillus*

* Scallops. † Radiated razor-ſhell.

‡ Alpha cockle. § Double cock's-comb.

|| Hammer oyſter; theſe were pretty large, but many broken, and ſome covered by a calcareous cruſt. It is very probable that among thoſe there may be ſome precious *white* ones. ¶ Swallow muſcle.

*puſillus Linn.**, *mitra epiſcopalis Linn.*, *lepas ſtriata Pennanti, (vide Zool. Brit.) patella tricarinata Linn.*, *bulla perfecta maculata* †, *harpa nobilis, porcellana ſalita Rumph.* ‡, *ſtrombus ſcorpio,* and other of inferior kinds. Amongſt the *zoophytes,* many valuable ſpecies of *ſpongiæ, corallinæ, ſatulariæ,* &c. a great variety of ſea-ſtars, and other marine productions, that cannot be preſerved in ſpirits, but ſhould be deſcribed on the ſpot. Theſe, as well as the deſcription of the different animals inhabiting the ſhells, are the more worthy of our attention, and deſerve farther inveſtigation, as we are yet very deficient in this branch of natural hiſtory.

During the fiſhing ſeaſon, the deſert, barren place Condatchey offers to our view a ſcene equally novel and aſtoniſhing. A heterogeneous mixture of thouſands of people, of different colours, countries, caſts, and occupations; the number of tents and huts erected on the ſea-ſhore, with their ſhops or bazars before each of them; and the many boats returning on ſhore in the afternoon, generally richly laden; all together form a ſpectacle entirely new to an European eye. Each owner runs to his reſpective boat as ſoon as it reaches the ſhore, in hopes of finding it fraught with immenſe treaſure, which is often much greater in imagination than in the ſhell; and though he is diſappointed one day, he relies with greater certainty on the next, looking forward to the fortune promiſed him by his ſtars, as he thinks it impoſſible for the aſtrological predictions of his Bráhmen to err.

To prevent riot and diſorder, an officer with a party of Malays is ſtationed here. They occupy a large ſquare, where they have a field-piece, and a flag-ſtaff for ſignals.

Here and there you meet with brokers, jewellers, and merchants, of all deſcriptions; alſo ſuttlers, offering proviſions and other articles to gratify the ſenſual appetite and luxury. But by far the greater number are occupied with the pearls. Some are buſily employed in aſſorting them; for which purpoſe they make uſe of ſmall braſs plates perforated with holes of different ſizes; others are weighing, and offering them to

* The wood-piercer. † Diving ſnail, (Grew, Muſ.)

‡ Salt-coury, Kl.

the purchaſer; while others are drilling or boring them, which they perform for a trifle.

The inſtrument theſe people carry about with them for this purpoſe is of a very ſimple conſtruction, but requires much ſkill and exerciſe to uſe it; it is made in the following manner: The principal part conſiſts of a piece of ſoft wood, of an obtuſe, inverted, conical ſhape, about ſix inches high and four in diameter in its plain ſurface; this is ſupported by three wooden feet, each of which is more than a foot in length. Upon the upper flat part of this machine are holes or pits for the larger pearls, and the ſmaller ones are beat in with a wooden hammer. On the right ſide of this ſtool, half a cocoa-nut ſhell is faſtened, which is filled with water. The drilling inſtruments are iron ſpindles, of various ſizes, adapted to the different dimenſions of the pearls, which are turned round in a wooden head by a bow. The pearls being placed on the flat ſurface of the inverted cone, as already mentioned, the operator, ſitting on a mat, preſſes on the wooden head of his inſtrument with the left hand, while, with his right, he moves the bow which turns round the moveable part of the drill; at the ſame time he moiſtens the pearl, occaſionally dipping the little finger of the ſame hand into the water of the cocoa-nut ſhell with a dexterity that can only be attained by conſtant practice.

Amongſt the crowd are found vagabonds of every deſcription, ſuch as Pandarams, Andee or Hindu monks, fakirs, beggars, and the like, who are impertinently troubleſome. Two of theſe wretches particularly attracted the attention of the mob, though their ſuperſtitious penance muſt have diſguſted a man of the leaſt reflection: one had a gridiron, of one foot and a half long and the ſame in breadth, faſtened round his neck, with which he always walked about, nor did he take it off either when eating or ſleeping; the other had faſtened round that member which decency forbids me to mention, a braſs ring, and fixed to it was a chain, of a fathom in length, trailing on the ground; the links of this chain were as thick as a man's finger, and the whole was exhibited in a moſt ſcandalous manner.

The peſtilential ſmell occaſioned by the numbers of putre-

 fying

fying pearl fishes, renders the atmosphere of Condatchey so insufferably offensive when the south-west wind blows, that it sensibly affects the olfactory nerves of any one unaccustomed to such cadaverous smells. This putrefaction generates immense numbers of worms, flies, muskitoes, and other vermin, all together forming a scene strongly displeasing to the senses.

Those who are not provided with a sufficient stock of money suffer great hardships, as not only all kinds of provisions are very dear, but even every drop of good water must be paid for. Those who drink the brackish water of this place are often attacked by sickness. It may easily be conceived what an effect the extreme heat of the day, the cold of the night, the heavy dews, and the putrid smell, must have on weak constitutions. It is therefore no wonder, that of those who fall sick many die, and many more return home with fevers, fluxes, or other equally fatal disorders.

The many disappointments usually experienced by the lower classes of men in particular, make them often repent of their coming here. They are often ruined, as they risk all they are worth to purchase pearl shells: however, there are many instances of their making a fortune beyond all expectation. A particular circumstance of this kind fell within my own observation: a day labourer bought three oysters * for a copper fanam, (about the value of two-pence,) and was so fortunate as to find one of the largest pearls which the fishery produced this season.

The donies appointed for the fishery are not all procured at Ceylon; many came from the coasts of Coromandel and Malabar, each of which has its distinguishing number. About ten o'clock at night a gun is fired as a signal, when they sail from Condatchey with an easterly or land wind, under the direction of a pilot. If the wind continues fair, they reach the bank before day, and begin diving at sun-rise, which they continue till the west or sea-breeze sets in, with which they return. The moment they appear in sight, the

* The East India pearl shell is well known to be the *matrix perlarum* (mother-of-pearl) of Rumphius, or the *Mytilus margaritiferus* of Linneus, consequently, the general term pearl-oyster must be erroneous: however, as it has long been in common use, I hope to be excused for continuing it.

colours

colours are hoisted at the flag-staff, and in the afternoon they come to an anchor; so that the owners of the boats are thereby enabled to get their cargoes out before night, which may amount to 30,000 oysters, if the divers have been active and succesful.

Each boat carries twenty-one men and five heavy diving-stones, for the use of ten divers, who are called in Tamul *kooly kárer*; the rest of the crew consists of a tandel, or head boatman, and ten rowers, who assist in lifting up the divers and their shells.

The diving-stone is a piece of coarse granite, a foot long, six inches thick, and of a pyramidical shape, rounded at the top and bottom. A large hair rope is put through a hole in the top. Some of the divers use another kind of stone, shaped like a half moon, to bind round their belly, so that their feet may be free. At present these are articles of trade at Condatchey. The most common, or pyramidical stone, generally weighs about thirty pounds. If a boat has more than five of them, the crew are either corporally punished or fined.

The diving, both at Ceylon and at Tutucorin, is not attended with so many difficulties as authors imagine. The divers, consisting of different casts and religions, (though chiefly of Parrawer * and Mussulmans,) neither make their bodies smooth with oil, nor do they stop their ears, mouths, or noses with any thing, to prevent the entrance of salt water. They are ignorant of the utility of diving-bells, bladders, and double flexible pipes. According to the injunctions of the shark conjurer they use no food while at work, nor till they return on shore and have bathed themselves in fresh water. These Indians, accustomed to dive from their earliest infancy, fearlessly descend to the bottom, in a depth of from five to ten fathoms, in search of treasures. By two cords a diving-stone and a net are connected with the boat. The diver, putting the toes of his right foot on the hair rope of the diving-stone, and those of his left on the net, seizes the two cords with one hand, and, shutting his nostrils with the other, plunges into the water. On reaching the bottom, he hangs the net round his neck, and collects into it the pearl shells

* Fishermen of the Catholic religion.

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as faſt as poſſible during the time he finds himſelf able to remain under water, which uſually is about two minutes. He then reſumes his former poſture, and making a ſignal, by pulling the cords, he is immediately lifted into the boat. On emerging from the ſea, he diſcharges a quantity of water from his mouth and noſe, and thoſe who have not been long enured to diving frequently diſcharge ſome blood; but this does not prevent them from diving again in their turn. When the firſt five divers come up, and are reſpiring, the other five are going down with the ſame ſtones. Each brings up about one hundred oyſters in his net, and, if not interrupted by any accident, may make fifty trips in a forenoon. They and the boat's crew get generally from the owner, inſtead of money, a fourth of the quantity which they bring on ſhore; but ſome are paid in caſh, according to agreement.

The moſt ſkilful divers come from Coiliſh, on the coaſt of Malabar; ſome of them are ſo much exerciſed in the art, as to be able to perform it without the aſſiſtance of the uſual weight, and for a handſome reward will remain under water for the ſpace of ſeven minutes: this I ſaw performed by a Caffry boy, belonging to a citizen at Karical, who had often frequented the fiſheries of theſe banks. Though Dr. Halley deems this impoſſible, daily experience convinces us, that, by long practice, any man may bring himſelf to remain under water above a couple of minutes. How much the inhabitants of the South Sea Iſlands diſtinguiſh themſelves in diving we learn from ſeveral accounts; and who will not be ſurpriſed at the wonderful Sicilian diver Nicholas, ſurnamed the Fiſh * ?

Every one of the divers, and even the moſt expert, entertain a great dread of the ſharks, and will not, on any account, deſcend until the conjurer has performed his ceremonies. This prejudice is ſo deeply rooted in their minds, that the government was obliged to keep two ſuch conjurers

* According to Kircher, he fell a victim amongſt the Polypes in the gulph of Charybdis, on his plunging, for the ſecond time, in its dangerous whirlpool, both to ſatisfy the curioſity of his king, Frederic, and his inclination for wealth. I will not pretend to determine how far this account has been exaggerated.

always

always in their pay, to remove the fears of their divers. Thirteen of theſe men were now at the fiſhery from Ceylon and the coaſt, to profit by the ſuperſtitious folly of theſe deluded people. They are called in Tamul *Pillal Kadtar*, which ſignifies one who binds the ſharks, and prevents them from doing miſchief.

The manner of enchanting conſiſts in a number of prayers learned by heart, that nobody, probably not even the conjurer himſelf, underſtands, which he, ſtanding on the ſhore, continues muttering and grumbling from ſun-riſe until the boats return: during this period they are obliged to abſtain from food and ſleep, otherwiſe their prayers would have no avail; they are, however, allowed to drink, which privilege they indulge in a high degree, and are frequently ſo giddy as to be rendered very unfit for devotion. Some of the conjurers accompany the divers in their boats; which pleaſes them very much, as they have their protectors near at hand. Nevertheleſs, I was told, that in one of the preceding fiſheries a diver loſt his leg by a ſhark; and when the head conjurer was called to an account for the accident, he replied, that an old witch had juſt come from the coaſt, who, from envy and malice, had cauſed this diſaſter by a counter-conjuration, which made fruitleſs his ſkill, and of which he was informed too late: but he afterwards ſhewed his ſuperiority by enchanting the poor ſharks ſo effectually, that, though they appeared in the midſt of the divers, they were unable to open their mouths. During my ſtay at Condatchey, no accident of this kind happened. If a ſhark is ſeen, the divers immediately make a ſignal, which on perceiving, all the boats return inſtantly. A diver who trod upon a hammer-oyſter, and was ſomewhat wounded, thought he was bit by a ſhark, conſequently made the uſual ſignal; which cauſed many boats to return; for which miſtake he was afterwards puniſhed.

The owners of the boats * ſometimes ſell their oyſters, and at other times open them on their own account. In the latter caſe, ſome put them on mats in a ſquare, ſurrounded with a

* Theſe are the individuals which farm one or more boats from the renter; and though they are in poſſeſſion of them only during the fiſhery, they are commonly called the owners of the boats.

fence;

ſence; others dig holes of almoſt a foot deep, and throw them in till the animal dies; after which they open the ſhells, and take out the pearls with more eaſe. Even theſe ſquares and holes are ſold by auction after the fiſhery is finiſhed, as pearls often remain there mixed with the ſand.

In ſpite of every care, tricks in picking out the pearls from the oyſters can hardly be prevented. In this the natives are extremely dexterous. The following is one mode they put in practice to effect their purpoſe: when a boat-owner employs a number of hired people to collect pearls, he places over them an inſpector of his own, in whom he can confide; theſe hirelings previouſly agree that one of them ſhall play the part of a thief, and bear the puniſhment, to give his comrades an opportunity of pilfering. If one of the gang happens to meet with a large pearl, he makes a ſign to his accomplice, who inſtantly conveys away one of ſmall value, purpoſely, in ſuch a manner as to attract notice. On this the inſpector and the reſt of the men take the pearl from him: he is then puniſhed, and turned out of their company. In the mean time, while he is making a dreadful uproar, the real thief ſecures the valuable pearl, and afterwards the booty is ſhared with him who ſuffered for them all. Beſides tricks like theſe, the boat-owners and purchaſers often loſe many of the beſt pearls while the dony is returning from the bank; for, as long as the animal is alive, and untouched, the ſhells are frequently open near an inch; and if any of them contain a large pearl, it is eaſily diſcovered, and taken out by means of a ſmall piece of ſtiff graſs, or bit of ſtick, without hurting the pearl fiſh. In this practice they are extremely expert. Some of them were diſcovered whilſt I was there, and received their due puniſhment.

Gmelin aſks, if the animal of the *mytilus margaritiferus* is an *aſcidia?* See Linn. Syſt. Nat. tom. I. p. vi. 3350. This induces me to believe that it has never yet been accurately deſcribed; it does not reſemble the *aſcidia* of Linnæus, and may, perhaps, form a new genus. It is faſtened to the upper and lower ſhells by two white flat pieces of muſcular ſubſtance, which are called by Houttuin * *ears*, and extend

* Vide Houtt. Nat. Hiſt. Vol. I. p. xv. p. 381, ſeq.

about two inches from the thick part of the body, growing gradually thinner. The extremity of each ear lies looſe, and is ſurrounded by a double brown fringed line. Theſe lie almoſt the third of an inch from the outer part of the ſhell, and are continually moved by the animal. Next to theſe, above and below, are ſituated two other double fringed moveable ſubſtances, like the branchiæ of a fiſh. Theſe ears and fringes are joined to a cylindrical piece of fleſh of the ſize of a man's thumb, which is harder and of a more muſcular nature than the reſt of the body. It lies about the centre of the ſhells, and is firmly attached to the middle of each. This, in fact, is that part of the pearl fiſh which ſerves to open and ſhut the ſhells. Where this column is faſtened, we find on the fleſh deep impreſſions, and on the ſhell various nodes of round or oblong forms, like imperfect pearls. Between this part and the hinge (*cardo*) lies the principal body of the animal, ſeparated from the reſt, and ſhaped like a bag. The mouth is near the hinge of the ſhell, enveloped in a veil, and has a double flap or lip on each ſide; from thence we obſerve the throat (*œſophagus*) deſcending like a thread to the ſtomach. Cloſe to the mouth there is a curved browniſh tongue, half an inch in length, with an obtuſe point; on the concave ſide of this deſcends a furrow, which the animal opens and ſhuts, and probably uſes to convey food to its mouth *. Near its middle are two blueiſh ſpots, which ſeem to be the eyes. In a pretty deep hole, near the baſe of the tongue, lies the beard (*byſſus*), faſtened by two fleſhy roots, and conſiſting of almoſt one hundred fibres, each an inch long, of a dark green colour, with a metallic luſtre;

* The depth at which the pearl fiſh generally is to be found, hindered me from paying any attention to the locomotive power, which I have not the leaſt doubt it poſſeſſes, uſing for this purpoſe its tongue. This conjecture is ſtrengthened by the accurate obſervations made on muſcles by the celebrated Reaumur, in which he found that this body ſerves them as a leg or arm, to move from one place to another. Though the divers are very ignorant with regard to the œconomy of the pearl fiſh, this changing of habitation has been long ſince obſerved by them. They allege, that it alters its abode, when diſturbed by an enemy, or in ſearch of food. In the former caſe they ſay it commonly deſcends from the ſummit of the bark to its declivity.

they

they are undivided, parallel, and flattened. In general, the *byſſus* is more than three quarters of an inch without the cleft (*rima*); but if the animal is diſturbed, it contracts it conſiderably. The top of each of theſe threads terminates in a circular gland or head, like the *ſtigma* of many plants. With this *byſſus* they faſten themſelves to rocks, corals, and other ſolid bodies; by it the young pearl fiſh cling to the old ones, and with it the animal procures its food, by extending and contracting it at pleaſure. Small ſhell fiſh, on which they partly live, are often found clinging to the former. The ſtomach lies cloſe to the root of the beard, and has, on its lower ſide, a protracted obtuſe point. Above the ſtomach are two ſmall red bodies, like lungs; and from the ſtomach goes a long channel or gut, which takes a circuit round the muſcular column above mentioned, and ends in the anus, which lies oppoſite to the mouth, and is covered with a ſmall thin leaf, like a flap. Though the natives pretend to diſtinguiſh the ſexes by the appearance of the ſhell, I could not find any genitalia. The large flat ones they call males, and thoſe that are thick, concave, and vaulted, they call females, or *pedoo-chippy*; but, on a cloſe inſpection, I could not obſerve any viſible ſexual difference.

It is remarkable that ſome of theſe animals are as red as blood, and that the inſide of the ſhell has the ſame colour, with the uſual pearly luſtre; though my ſervants found a reddiſh pearl in an oyſter of this colour, yet ſuch an event is very rare. The divers attribute this redneſs to the ſickneſs of the pearl fiſh, though it is moſt probable that they had it from their firſt exiſtence. In the ſhade they will live twenty-four hours after being taken out of the water. This animal is eaten by the lower claſs of Indians, either freſh in their curries, or cured by drying; in which ſtate they are exported to the coaſt; though I do not think them by any means palatable.

Within a mother-of-pearl ſhell I found thirteen *murices nudati* (*vide* Chemnitz's New Syſtem, Cabt. Vol. XI. tab. 192, f. 1851 and 1852), the largeſt of which was three quarters of an inch long; but, as many of them were putrid, and the pearl fiſh itſelf dead, I could not aſcertain whether they had

crept in as enemies, or were drawn in by the animal itſelf. At any rate turtles and crabs are inimical to the animals, and a ſmall living crab was found in one of them.

The pearls are only in the ſofter part of the animal, and never in that firm muſcular column above mentioned. We find them in general near the earth, and on both ſides of the mouth. The natives entertain the ſame fooliſh opinion concerning the formation of the pearl which the ancients did: they ſuppoſe them formed from dew-drops in connection with ſun-beams. A Bráhmen informed me that it was recorded in one of his Sanſcrit books, that the pearls are formed in the month of May at the appearance of the Soatee ſtar (one of their twenty-ſeven conſtellations), when the oyſters come up to the ſurface of the water to catch the drops of rain. One of the moſt celebrated conchologiſts* ſuppoſes that the pearl is formed by the oyſter in order to defend itſelf from the attacks of the *pholades* and *boreworms*. But we may be aſſured that in this ſuppoſition he is miſtaken; for, although theſe animals often penetrate the outer layers of the pearl ſhell, and there occaſion hollow nodes, yet, on examination, it will be found that they are never able to pierce the firm layer with which the inſide of the ſhell is lined. How can the pearls be formed as a defence againſt exterior worms, when, even on ſhells that contain them, no worm-holes are to be ſeen? It is therefore more probable theſe worms take up their habitations in the nodes in order to protect themſelves from the attacks of an enemy, than that they are capable of preying on an animal ſo well defended as the pearl fiſh is. It is unneceſſary to repeat the various opinions and hypotheſes of other modern authors; it is much eaſier to criticiſe them, than to ſubſtitute in their place a more rational theory. That of Reaumur, mentioned in the memoirs of the French Academy for 1712, is the moſt probable, *viz.* that the pearls are formed like bezoars and other ſtones in different animals, and are apparently the effects of a diſeaſe. In ſhort, it is very evident that the pearl is formed by an extravaſation of a glutinous juice, either within the body, or on the ſurface of the animal: the former caſe is the moſt com-

* The Rev. Mr. Chemnitz at Copenhagen.

mon. Between one and two hundred pearls have been found within one oyſter. Such extravaſations may be cauſed by heterogeneous bodies, ſuch as ſand, coming in with the food, which the animal, to prevent diſagreeable friction, covers with its glutinous matter, and which, as it is ſucceſſively ſecreted, forms many regular lamellæ in the manner of the coats of an onion, or like different ſtrata of bezoars, only much thinner; this is probable, for if we cut through the centre of a pearl, we often find a foreign particle, which ought to be conſidered as the nucleus, or primary cauſe of its formation. The looſe pearls may originally have been produced within the body, and, on their increaſe, may have ſeparated and fallen into the cavity of the ſhell. Thoſe compact ones, fixed to the ſhells, ſeem to be produced by ſimilar extravaſation occaſioned by the friction of ſome roughneſs on the inſide of the ſhell. Theſe and the pearl-like nodes have a different aſpect from the pearls, and are of a darker and bluer colour. In one of the former I found a pretty large, true, oval pearl, of a very clear water; while the node itſelf was of a dark blueiſh colour. The yellow or gold-coloured pearl, is the moſt eſteemed by the natives; ſome have a bright red luſtre; others are grey, or blackiſh, without any ſhining appearance, and of no value. Sometimes, when the grey lamella of a pearl is taken off, under it is found a beautiful genuine one; but it oftener happens that, after having ſeparated the firſt coat, you find a worthleſs impure pearl. I tried ſeveral of them, taking one lamella off after another, and found clear and impure by turns; and in an impure pearl I met with one of a clear water, though in the centre of all I found a foreign particle. The largeſt and moſt perfect pearl which I ſaw during my ſtay at Condatchey was about the ſize of a ſmall piſtol bullet, though I have been told ſince my departure many others of the ſame ſize have been found. The ſpotted and irregular ones are ſold cheap, and are chiefly uſed by the native phyſicians as an ingredient in their medicines.

We may judge with greater or leſſer probability, by the appearance of the pearl ſhell, whether they contain pearls or not. Thoſe that have a thick calcareous cruſt upon them,

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to which *ſerpulæ* (ſea tubes) *Tubuli marini irregulariter intorti, Criſta-gali Chamar lazuras, Lepas tintinabulum, Madreporee, Millipore, Cellipore, Gorgontæ, Spongiæ,* and other Zoophytes are faſtened, have arrived at their full growth, and commonly contain the beſt pearls; but thoſe that appear ſmooth, contain either none, or ſmall ones only.

Were a naturaliſt to make an excurſion for a few months to Manâr, the ſmall iſland near Jafna, and the adjacent coaſt, he would diſcover many natural curioſities ſtill buried in obſcurity, or that have never been accurately deſcribed.

Indeed no place in the Eaſt Indies abounds more with rare ſhells than theſe; for there they remain undiſturbed, by being ſheltered from turbulent ſeas, and the fury of the ſurf. I will juſt name a few of them; viz. *Tellina foliaca* Linn *, *Tell Spenglerii, Arca culculata* †, *Arca Noæ, ſolen anatinus* Linn. *Oſtrea Iſognomum, Terebullum, albidum, ſtriatum, Turbo ſcalaris* ‡, *Bula volva* Linn. §, *Vexillum ingritarum, &c.* Amongſt the beautiful conc ſhells; *conus thalaſſiarchus Anglicanus cullatus* ||, *amadis thaſſiarchus con. generalcis* Linn. *c. capitaneus* ¶, *c. miles* **, *c. ſtercus muſcarum* ††, *c. reteaureum, c. glaucus* ‡‡, *c. cereola, regia corona, murus lapidius, cauda erminea, ſocietas cordium.* There are many others beſides thoſe already mentioned, equally valuable and curious.

The great ſucceſs of the Rev. Doctor John in conchology when at Tutucorin, and aſſiſted by G. Angelbeck, with a boat and divers; and the capital collections made by his agents, whom he afterwards ſent there with the neceſſary inſtructions and apparatus, may be ſeen in Chemnitz's elegant Cabinet of Shells in 4to, (with illuminated plates); and how many new ſpecies of Zoophytes he diſcovered, we learn from another German work by Eſper at Erlangen, the third volume of which is nearly finiſhed.

* The golden tong. † Mounkſcape. ‡ Royal ſtaircaſe.
§ Weaver's ſhuttle. || Red Engliſh admiral.
¶ Green ſtamper. ** Garter ſtamper.
†† Great ſand ſtamper. ‡‡ Capſ. Gottw.

VII. *Account*

VII. *Account of a new Method of Bleaching Cotton.* By C. Chaptal, *Member of the National Inftitute**.

THE happy applications which C. Berthollet has made of the oxygenated muriatic acid, in the bleaching of ftuffs manufactured from vegetable fubftances, feems to have carried this art very near to perfection; but this method is not every where attended with the fame degree of economy. Befides, the procefs requires very fkilful hands, that the ftuffs may not be deftroyed by too corrofive leys, or leys improperly employed; and therefore we ought not to omit making known other proceffes, in order that manufacturers may choofe thofe which they may think moft beneficial. I fhall therefore here defcribe a procefs, both fimple and economical, for bleaching cotton yarn.

At the diftance of about one foot four inches from the grate of a common furnace, place a copper kettle of a round form, one foot and a half in depth and four feet in diameter, and fix it in that pofition. The brim of this kettle, about fix inches in breadth, being bent outwards, will reft on the lateral edges of the mafon-work of the furnace. The remainder of the furnace muft be conftructed of cut ftone, in the form of an oval boiler, fix feet in height, and in breadth, meafured from the centre, five feet. The upper part of the furnace has a round aperture, in diameter about a foot and a half. This aperture may be clofed by means of a moveable ftone, or a copper lid made for the purpofe. On the edge of the copper kettle, which forms the bottom of this fpecies of digefter, place a kind of grate confifting of wooden bars, brought pretty near to each other in order that the cotton laid upon them may not fall through, and fufficiently ftrong to fuftain the weight of about 1600 pounds. When this apparatus has been conftructed, impregnate the cotton difpofed in bundles, with a flight folution of foda, rendered cauftic by lime: this operation muft be performed in a wooden or ftone trough, in which the cotton may be trod

* From the *Bulletin des Sciences*, Vol. II. No. 6.

upon

upon by fecuring the feet with wooden fhoes. When the alkaline liquor has well penetrated the cotton in an equal manner, it is to be carried to the boiler, and placed on the wooden grate already mentioned. The fuperfluous liquor will run down between the bars, and form a liquid ftratum, which will permit the mafs to be heated without any danger of burning the cotton or the bottom of the boiler. To form the alkaline ley, foda of Alicant may be employed equal to a tenth part of the weight of the cotton on which you operate; and in a boiler fuch as I have defcribed, about 800 pounds of cotton may be fubjected to the procefs at one time. At the moment when the cotton is introduced and arranged in the boiler, the aperture at top is fhut, by its ufual covering, as clofely as poffible, in order that the vapours may affume a greater degree of heat, and re-act with more force on the cotton. The fire of the furnace is then to be kindled *, and the ley muft be kept in a ftate of flight ebullition for from twenty to thirty-fix hours. It is then fuffered to cool, and the cotton being carefully wafhed, muft be expofed on the grafs for two or three days, extending it on poles in the day-time, and fpreading it out on the grafs during the night. The cotton will then have acquired a fuperb degree of whitenefs; and if, by chance, any parts of the cotton are ftill coloured, it muft be put into the kettle a fecond time, or be expofed a few days longer on the grafs. Thefe fhades in the bleached cotton are owing in particular to its not having been all equally and completely impregnated with the ley; they may arife alfo from the arrangement of the cotton in the boiler, if it has been heaped up too much in certain points. When it is judged that the ley has been exhaufted by ebullition, the cover of the kettle is to be taken off, and the dry cotton muft be befprinkled with a new folution of foda; without this precaution it might run the danger of being burnt. It might readily be judged, by eftimating the fubftances and time employed in this operation, that it is attended with economical advantages; but we have a more fimple method of af-

* In giving the above dimenfions, I have fuppofed the fuel ufed to be coal: if wood is burnt, the dimenfions muft be varied. In the latter cafe the bottom of the kettle would be too high above the bottom of the furnace.

certaining

certaining the fact, that is, the low price at which cotton is bleached in all those manufactories where this process is used. In the south of France, where this method is at present pretty generally adopted, eighty pounds of cotton are bleached for about seven shillings sterling. This process was brought to us from the Levant some time after the introduction of the process for dyeing the Adrianople red. It has been practised for some time, but kept a secret till the present moment, and still known by the name of *blanchiment à la fumée*, bleaching by smoke.

I do not know that this method has been applied to the bleaching of spun flax or hemp. It would, however, be worth while to try the experiment: stronger leys and longer ebullition, no doubt, must be employed; but it is by experience alone that we can acquire information on the subject.

VIII. *On the Method of preparing Inks that will withstand the Action of the Oxygenated Muriatic Acid. By* A. BOSSE, *of Hamburgh* *.

INTRODUCTION.

FOR illustrating the history of this subject, which has been fully treated of in the New Hanoverian Magazine, the following information may be necessary:—Dr. Lentin, in his paper containing cautions in regard to the misapplication of the oxygenated muriatic acid †, observed, that with the help of this acid our common ink could be obliterated without the least injury to the paper which had been written with it, if the paper were first drawn through diluted oxygenated muriatic acid; then through diluted sulphurous acid; and lastly, through water. At the same time he added some information which seemed to prove that this property had been employed in France several times to answer private purposes, and to the prejudice of others. Soon after appeared a

* From *Scherer's Algemeines Journal der Chemie*, Vol. II. No. 10. The Introduction is by Dr. Scherer.

† Hannov. Mag. 1797, part 71.

paper

paper by Philip Chriſtian Pitel of Minden *, in which he recommended an ink, diſcovered by him, which was indeſtructible, and could be obliterated neither by the oxygenated muriatic acid, nor by any other corroſive ſubſtance. This induced M. Wehrs to examine this ink, and the ſame thing was undertaken by M. Gruner †. According to their experiments, this ink, however, was obliterated in the courſe of nine hours by the oxygenated muriatic acid, and alſo by cauſtic pot-aſh; but at the ſame time an opinion entirely oppoſite was announced by Dr. Lentin ‡, M. Thorey §, and M. Wiegleb ||, who all, in conſequence of their having employed the ſame teſt, declared the ink to be indeſtructible. Theſe different opinions are therefore directly contrary to each other. M. Gruner found that the ink could be deſtroyed by the oxgenated muriatic acid, and by cauſtic alkali: M. Thorey obſerved, that its blackneſs was only leſſened by the oxygenated muriatic acid, and by the vapour of that acid. He found, however, as M. Gruner did, that the cauſtic alkali diſſolved the ink entirely from the paper, but that, like ſeveral acids, it deſtroyed the paper. M. Lentin and Wiegleb deduced from their experiments, thoſe of the former being made with oxygenated muriatic acid, and thoſe of the latter with the ſame, as well as ſeveral other acids, and even cauſtic alkali, that this ink was entirely indeſtructible. Theſe contradictory reſults M. Weſtrumb endeavoured to reſolve in his examination of Pitel's indeſtructible ink, which he found to be only common ink mixed with indigo; and he propoſed the following mixture for an ink which could not be deſtroyed:—Boil 1 oz. of Braſil wood, and 3 ozs. of pulveriſed galls, with 46 oz. of water; ſtrain the liquor, which muſt be boiled down to 32 ozs., and pour it, ſtill warm, over $1\frac{1}{2}$ oz. of perfectly pure ſulphate of iron, $1\frac{1}{4}$ oz. of gum arabic, and $\frac{1}{4}$ oz. refined ſugar. When theſe ingredients are diſſolved, add from 1 to $1\frac{1}{4}$ oz. good indigo ground exceedingly fine, and $\frac{1}{4}$ oz. of purified lamp black.

* Hannov. Mag. 1797, part 77. † Ibid. ‡ Ibid. p. 1223.
§ Ibid. 1797, part 97. || Reich's Anzeiger 1797, No. 297.

M. Bosse's

M. Bosse's Paper.

It is well known that M. Pitel, of Minden, gave the firft account of a kind of ink which withftood the oxygenated muriatic acid, at the defire of Dr. Lentin, who excited the attention of the public to various deceptions practifed at Paris with common ink by means of the oxygenated muriatic acid. As this ink confifted merely of a decoction of logwood and galls with water, in which fulphate of iron, gum arabic, and fugar had been diffolved, and with which indigo and lamp black had been mixed mechanically, according to the analyfis of M. Weftrumb, it may be eafily explained why it exhibited fuch different appearances when brought to the teft. M. Gruner, apothecary in Hanover, as appears by his letter to the editor of the Hanoverian Magazine, was able to obliterate it entirely by the muriatic acid; while, on the other hand, its indeftructibility by that acid is confirmed by M. Wiegleb and Thorey. Both are in the right: as foon as the ink is well ftirred round, it withftands this acid; but, if not ftirred, this acid diffolves the black oxyd of iron, and the paper in the places wrote upon is reftored to its priftine ftate. Now, though this ink, when ftirred round, does not entirely fail of its object, this circumftance, however, may be often forgotten; and this the more readily, as even without ftirring it has all the blacknefs when ufed of common ink, on account of the oxyd of iron which it contains. Since that time I have made experiments in order to produce an ink which might withftand the oxy-muriatic acid, and yet contain no iron. I have been able to accomplifh this, as well as the compofition of fome other inks which contain iron, though it does not make their chief component parts, but ferves only to give the ink a black colour. I made experiments alfo with the juice of green plants, according to the procefs by which M. Murray made an indeftructible kind of ink from the fame fubftances. Iron muft not be the chief component part of an ink deftined to withftand the muriatic acid, becaufe in the ftate of a black oxyd it is eafily diffolved by the acid. But as it acts on vegetable colours only when they are capable of taking up oxygen, and does not deftroy

 them,

them, but produces by its action a modification of the colour, they are exceedingly proper for this purpose. As none of these, however, were found entirely black, I was obliged to employ some mineral body which might be black and yet hold a great deal of oxygen. The most part of the metallic oxyds of a black colour contain very little oxygen, except that of manganese, which contains a great deal. I made choice therefore of this oxyd, and found, after repeated experiments, that it answered the intended purpose.

The oxy-muriatic acid dissolves all metals in a metallic form as well as metallic calces combined with a small quantity of oxygen, but not those which contain a great deal, as it has itself an excess of it. It is therefore capable,

1. Of dissolving metals in a metallic state, as the metals take up its superfluous oxygen, by which, as is well known, they become soluble in acids.

2. Such metallic oxyds as contain little oxygen are capable of abstracting it from acids, and they then dissolve in the acids, which then contain less oxygen.

The grounds which induced me to employ oxyd of manganese for preparing an ink capable of withstanding the oxy-muriatic acid, were as follows:—

1. Its black colour; 2. its containing a great quantity of oxygen, by which it is insoluble in the muriatic acid; 3. because it possesses so great an affinity for oxygen, that whenever it has lost any of its original quantity by being brought to a red heat, or exposed to the action of acids, it immediately draws it from the atmosphere, and again becomes black. I shall now describe the method of preparing this ink:—Boil 1 oz. of Brazil wood with 12 ozs. of water for a quarter of an hour; add $\frac{1}{2}$ oz. of alum: evaporate the whole to 8 ozs., and mix with the liquor 1 oz. of exceedingly soft, finely pulverised manganese, mixed up with $\frac{1}{2}$ oz. of pulverised gum arabic.

Brazil wood alone, by mere boiling, gives an ink not altogether unfit for use. Acids, less abundant in oxygen, such as the nitrous and muriatic acid, naturally exercise an action on this ink; but as these change the paper in a perceptible manner, deception is not to be apprehended.

Indigo also affords an ink that withstands the muriatic acid:

acid: it therefore makes a chief component part of that prepared by Pitel, but it is mixed in it only mechanically. The following is a preſcription for preparing an ink of the like kind, but in which the indigo is actually diſſolved.

Boil 1 oz. of Brazil wood, and 3 ozs. of coarſely pulveriſed galls, with 9 ozs. of vinegar and as much water, for the ſpace of eight minutes: in the liquor, after being ſtrained, diſſolve $1\frac{1}{2}$ oz. of ſulphat of iron and 1 oz. of gum arabic, and then add to the whole a ſolution of $\frac{1}{2}$ oz. of indigo in 1 oz. of concentrated ſulphuric acid.

The oxy-muriatic acid diſſolves the oxyd of iron in this ink, but the indigo remains undecompoſed. The principal point to be attended to is, that the ink may contain ſuch matters as are not affected by the oxy-muriatic acid: with other ſubſtances leſs deception is to be dreaded.

In the laſt place, I ſhall mention an ink made with the principal ingredients of common ink, but in preparing which, inſtead of the uſual liquids, I employed the expreſſed juice of ſome plant: the fitteſt for this purpoſe I found to be the leaves of the caper ſpurge, *Euphorbia lathyris* Linn.; the common holly, *Sambucus niger*, and common graſs.

IX. *Sketch of the Hiſtory of Mining in Devon and Cornwall. By Mr.* John Taylor *jun. Miner, Taviſtock. Commumunicated by the Author.*

It muſt be regretted that a ſubject of ſo great national importance, undertakings which engage ſo large a capital, and give employment to ſo many hands, as working the various mines in theſe kingdoms, ſhould gain but little attention from any but thoſe immediately concerned in them, and near the places in which they are ſituated; it is the more to be wiſhed that men of ſcience ſhould devote ſome thought to theſe concerns, as thoſe, under whoſe management moſt of theſe works are carried on, are often not men of ſufficient ability or ſcience to ſtrike out capital improvements. To be a good miner requires an active mind, with induſtry and ſtrict obſervation; theſe ſhould be accompanied by ſome general

general knowledge, at leaſt, of practical mineralogy, chemiſtry, mechanics, hydraulics, &c., and ſuch a knowledge of principles as might lead to improvements in the practical part of his buſineſs. It would be unreaſonable to expect to find theſe qualifications general at preſent, but the exiſting ignorance might ſurely be ſoon removed, if, the attention of men of ſcience being turned to the ſubject, they would take pains to point out to the practical miner the improvements of which his operations are ſuſceptible.

If it be true, as was aſſerted in the number of the Philoſophical Magazine for February laſt, "that the pooreſt mine in Cornwall is worked at a greater expence than the richeſt one on the Continent," much is to be done; although to work a mine rich or poor to the ſame extent muſt require the ſame expence. From my own experience I cannot aſſent to what is aſſerted, in the ſame paragraph, of the captains or directors of the Corniſh mines; I can bear teſtimony to there being among them men of ability, obſervation, and liberality; of all the preſent practical knowledge of the ſubject many of them are perfect maſters, and have not often, I think, that deſire of excluding interference which is imputed to them.

Mining is a ſubject requiring ſtudy, and deſerving the attention of the philoſopher, as it tends to unveil ſome of the moſt hidden proceſſes of Nature, and to anſwer important ends to Society. On the Continent many of the moſt eminent men have not thought it beneath them to undertake the management of mines; and to this it is fair to impute the capital manner in which ſome foreign mines are reported to be conducted.

It muſt afford pleaſure to know that of late years many improvements in mining, as well as in moſt other arts connected with or depending on the ſciences, have taken place; and both the diſcovery of the ores, and their treatment after being brought to the ſurface, is, in moſt reſpects, carried on with greater certainty and ſkill, and conſequently more profit.

It may afford ſome amuſement to attend a little to the hiſtory of this ſource of the moſt ancient traffic of our iſland, and the cauſe, probably, of much of our civilization, it having attracted,

attracted, long ago, the viſits of other more enlightened nations.

The tin of Britain was known in diſtant parts of the world at a very remote period. It is generally believed that the Phœnicians were the nation principally engaged in trading with Britain for this article. Tin works were certainly carried on before iron was in uſe in England. Many tools of oak are now found which tradition among the tinners make to have belonged to the Saxons or Danes; but it is probable, for the reaſon before ſtated, that they were employed before the time of their having a footing in this country.

The greateſt quantity of tin was formerly, it is aſſerted, found on the foreſt of Dartmoor, in the county of Devon; and works to a vaſt extent muſt have been carried on there, if it be true, as we are told, that thirty thouſand men found employment upon this tract; for all the miners now in Cornwall do not amount to more than ten thouſand. It is, however, to be conſidered, that in the ancient mode of working, without the aid of machinery, many more hands muſt have been neceſſary than at preſent—I ſhould think more than in the proportion of three to one. All theſe works on Dartmoor were only on or near the ſurface: now, however, the lodes * or veins of tin found on this diſtrict are not valuable enough, or do not continue to ſuch a depth as to make them very profitable to work.

Almoſt all the tin procured in former ages was probably from ſtream-works, in bottoms or low grounds, where fragments of the ore, waſhed from the lodes in the neighbouring hills, ſubſide, and are ſeparated from the earth in a granulated form by waſhing. This, of courſe, is obtained without any ſubterranean work. In ſuch ſituations as theſe probably metals were firſt diſcovered, mixed merely with the upper ſoil, or lodging in clefts of rocks. Thus is gold found in America, in Africa, in the county of Wicklow in Ireland, and ſo has it been occaſionally diſcovered in Cornwall; and veins or lodes of this metal might doubtleſs be found near the places where ſuch depoſitions are met with.

The tracing and following lodes of ores into the earth is a

* The name given in Cornwall to veins in which ore is found.

more difficult task than coming at the metal in the way of stream-works; it requires more energy of mind, and a more advanced state of the arts. The difficulty that must attend keeping a mine, sunk to some depth, free from water by manual labour only, could not but prevent men, before the application of machinery, from diving deep into the bowels of the earth. We have, however, some instances where old workings are found at so great a depth as to be even now with difficulty kept dry by means of machinery; but these, though they may be counted ancient, were probably opened long subsequent to the origin of mining in this county.

There are many mines which could not possibly be worked without the aid of gunpowder, and, until the discovery of this powerful agent, underground operations must have been uncertain and difficult. The hammer and wedges of metal were probably the first instruments for splitting rocks (and they still continue, in the ground that will yield to them, to be much used in Cornwall), and the pick, or instrument for cleaving the ground, having a head for driving the wedges called by the miners *gads*, from a Cornish word *gedn*, a wedge. The form of these instruments found in old works, I think, offers an evidence of their antiquity. A pick, which was found in Wheal * Unity tin mine, in Devon, in a part not worked certainly for more than eighty years, and which could not probably be reckoned to be less than 100 years old, does not differ materially from the form of that now used; while one discovered in old workings in Drake Walls mine, in the parish of Calstock in Cornwall, about ten miles distant from the other mine, is of so different a shape as to make one conclude, judging by the slow progression of changes effected on common instruments, that it is of much higher antiquity. Wedges of dry wood were also very ingeniously made use of, by driving them into the clefts, and then wetting them, so as to cause them to swell and force the ground asunder. Fire was an agent long ago employed for splitting the rocks; but the effects of gunpowder so far exceed any thing before made use of for such purposes, that its dis-

* In this part of the country the word *wheal*, signifying in the Cornish language *a work*, is generally prefixed to the proper names of mines.

covery

covery and application to works of this kind form a grand epoch in the hiſtory of mining. This, it appears, took place in Hungary or Germany about the year 1620, and was firſt introduced into England at the copper mine at Ecton, in Staffordſhire, about the year 1670, by German miners, brought over by Prince Rupert; but it was not in uſe in Somerſetſhire till 1684, after which, probably, the Corniſh miners became acquainted with this powerful aſſiſtant to their operations.

Tin was the firſt object of the Corniſh miner's ſearch, and I ſhall therefore firſt take notice of the hiſtory of the tin-works. It was probably firſt found, as I have obſerved, near the ſurface, and not in regular veins; almoſt all the low grounds in Cornwall, and I believe every bottom on Dartmoor foreſt in Devon, bear the marks of having been *ſtreamed*. The Romans, probably, intereſted themſelves in the working of the mines; indeed, one principal inducement to that people's turning their attention to this iſland, ſeems to have been the metals that were reported to them to be found here. The Saxons neglected theſe hidden treaſures, but the Normans worked them to great advantage. From that time to the end of the reign of John the mines were not profitable, and moſtly in the hands of the Jews. They revived in the time of his ſon Richard; but in the reign of Edward I. the Jews were baniſhed the kingdom, and the mines neglected. Edmund, the eldeſt ſon of the king, and earl of Cornwall, however, made ſome important alterations in the regulations of the tin-works by a charter, which was confirmed by Edward I. in the latter part of his reign. Indeed it is from this time that the peculiar laws and privileges relating to the Stannaries are chiefly to be dated. Mining infringed in ſome inſtances on property, and cauſed diſputes, beſides requiring indulgences not general; and thus caſes aroſe not cogniſable by the common law. In this way a peculiar code, ſpringing from cuſtom, took its riſe; and though this in ſome meaſure exiſted before, yet it was not till this period that it was confirmed by royal charter, and enforced by ſubſequent acts of parliament. It was by this charter that the bounding land to the purpoſe of tinners working on it, the duties to

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the earl of Cornwall, and the coinages of tin, or stamping with the earl's seal, were first established. Before the reign of Edward I. tinners worked in the earl's land only, paying him a fifteenth part of what they got, and they were not at all permitted to dig in sanctuary ground, churches, mills, houses, gardens, and so on; and if in working under they chanced to subvert any house, or to damage a high-way, they were obliged to make it good. When it became an object to search throughout any place or person's lands, a court also to determine cases relating to the tin-works became necessary; and this, adjudging under the authority of, and according to the code of laws before mentioned, was first established by Edward I. and is called the Stannary Court.

In Cornwall and Devon two different systems of Stannary laws now exist: those in Cornwall have been from time to time amended and corrected; but in Devon, where mining has for a long while slumbered, the laws continue in their original crude state. As the spirit, however, of searching into the bowels of the earth seems again to be reviving in this county, the laws, should they be called into action, will probably undergo some revision.

The duke of Cornwall had royal jurisdiction, and when the duchy came into the hands of the crown, which happened in the reign of Edward III. he made his eldest son, the Black Prince, the duke, and appointed it, with the revenue arising from it, to the sons and heirs apparent of the crown, though no son of the king can be duke of Cornwall but the first-born, even though heir apparent.

The Stannary laws were explained and confirmed, with some alterations at different times, till Arthur, eldest son of Henry VII. made certain constitutions relating to the Stannaries, which the tinners refused to observe: the king, after prince Arthur's death, seized the charter as forfeited, but granted a new one with fresh privileges, appointing all new laws relating to the tinners to be made by a parliament of their own body; and upon this footing the Stannaries remain.

Great powers have been at times assumed by this jurisdiction,

tion, it has even taken upon it the trial for felony in the cafe of a tinner; in general, however, the cafes where its authority interferes, are in difputes concerning tin-works or between tinners: it provides a fupply of water to a tin-work, giving a power to conduct it through any lands for that purpofe.

One of the improvements, with regard to tin particularly, was the invention of fmelting in reverberatory furnaces with pitcoal, inftead of the old *blowing-houfes*, as they were called, with charcoal fires. When this change was firft introduced is not perhaps certainly known: Dr. Watfon, in his Chemical Effays, ftates, that Becher refided in Cornwall fome years before he died, which was in the year 1682; and that he made many improvements in the working of mines and fluxing of metals, and introduced there the method of fmelting tin by the flame of pitcoal thrown upon the ore in a reverberatory furnace, inftead of the fire of wood or charcoal, in the way formerly made ufe of. He takes notice of this in the Dedication to Mr. Boyle of his *Alphabethum Minerale*, written at Truro in 1682, not long before he died. Pryce, in his *Mineralogia Cornubienfis*, ftates, that this mode was firft prompted by neceffity, and many experiments were made upon it by Sir Bevil Granville, of Stow, in the time of Charles I. though it was not effectually done till the reign of Queen Anne.

I find, in the revifion of the Stannary laws at the convocation of the parliament of Stannators affembled in the 26th year of the reign of George II. mention is made of the "ancient laws and conftitutions of the Stannaries relating to tin-blowers, and refining of tin in *blowing-houfes*, which was formerly the only method of refining of tin;" and it is faid, "of late years there has been another method found out of refining of tin by means of reverberatory furnaces, which is commonly called fmelting of tin."

Copper, of which fo much is now fent from this part of the country, was not an object attended to, till a comparatively late date, by the Cornifh miners: even in tin mines, which as they deepened produced copper, as is often the cafe, and where they needed to raife this ore, it was thrown by as of no value, going by the name of *poder*. Thofe who

live in the prefent more enlightened period, are now reaping profits left to them by the ignorance of their forefathers. This proves of what confequence it is to determine, if poffible, the value of all the fubftances paffing under the miner's obfervation.

Copper, however, was probably worked at a remote period in Wales, at the Parys mountain, which indeed is fuppofed to derive its name from the Celtic word *praas*, brafs or precious metal; and this would offer a proof of its having been worked by the ancient Britons. It was not attended to, or at leaft not well underftood, in England, till the reign of Queen Elizabeth, who paid great attention to the mines of the kingdom, and, by granting great privileges to Daniel Houghfetter, Chriftopher Schutz, and other Germans whom fhe invited into England, commenced and eftablifhed the highly valuable and important bufinefs of finding and purifying this ufeful metal. To thefe foreigners, too, is owing the flourifhing ftate of our brafs manufactories. In this reign Parys mine was granted to patentees, but was not worked, at leaft to any advantage, for a century and a half.

The copper mines of Cornwall now are works of great magnitude, and fome have been funk to an amazing depth, and are kept working at a vaft expence. The quantity of materials conftantly ufed forms an extenfive commerce to this part of the kingdom; and this, as well as the mines themfelves, gives employment to numbers.

The other metals are not found in Cornwall in great abundance. Lead, with filver contained in the ore, is found in fome places; and in Devonfhire, on the borders of this county, a very large mine at Bere afforded galena very rich in filver. It was formerly worked as a royal mine, but afterwards lay idle a long time. It has fince been feebly tried; but though a fteam-engine was on it, they could not go fo deep as it had been before funk without more power to draw the water, and not finding much valuable return, the mine has been lately ftopped again. Veins of lead ore fometimes run with copper lodes.

Iron is found in fome places, but not in large quantities; and coals being diftant, it will not repay the coft of procuring.

The ores of many of the ſemi-metals, as zinc, cobalt, arſenic, mangancſe, &c. are ſcattered in different places, and of late have received attention.

Mining has been much improved within this century. Till about a hundred years ago, the water was drawn from the mine by dint of human labour; but within theſe ſeventy years the application of hydraulic engines has become general. The introduction of the ſteam-engine formed a grand æra in the annals of mining; as in many ſituations no other power, that could be commanded, could poſſibly work the pumps neceſſary to raiſe the water from the bottoms: the conſequence has been, that many mines have been worked that were formerly abandoned as impoſſible to be proſecuted. Other improvements of late years have been creeping in; the dreſſing of ores, or properly ſeparating them from the earths, &c. in which they lie, is much better managed than formerly.

Great ſpirit is to be ſeen in the works in Cornwall; prodigious ſums are laid out in erecting powerful machinery, ſometimes even before any quantity of ore is ſeen. This often well repays thoſe who expend it, eſpecially if done under the direction of experienced and ſkilful managers, who are good judges of the appearances and ſymptoms on which is founded the expectation of finding a valuable return.

The very ſpirited and laudable exertions that are making, at a great riſk, to bring to the public uſe ſo valuable an article as the copper mines produce, have lately been damped by ſhutting up the channel of trade with regard to this commodity. The purpoſe for which this is done, namely, a reduction in the price, cannot in the end be anſwered; for even ſhould that for a time take place, the deep, and conſequently expenſive mines, and thoſe which produce ores of inferior quality, will probably be ſtopped; and thus, a ſmaller quantity coming to market, the price muſt again advance, perhaps higher than it now is. In the mean time, ſhould it operate to reduce the price, a valuable ſource of traffic will be loſt to the nation; the revenue will ſuffer; individuals be diſtreſſed; and numbers of people not uſed to any other way of life become deſtitute of the means of exiſtence.

X. *On*

X. *On the relative Proportions of Coals and Iron-Stones used at the Blast-Furnace, and of their proper Application to Use.* By Mr. DAVID MUSHET, *of the Clyde Iron-Works. Communicated by the Author.*

IN the ſmelting operation a juſt proportion and aſſociation of materials and mechanical conſtruction ought to be blended in order to produce the beſt poſſible effects. Under the former are comprehended the cokes, iron-ſtone, limeſtone, and blaſt; by the latter is underſtood the furnace, the power of the blowing-machine, or the compreſſion and velocity under which the air is diſcharged into the furnace, and the genius or mechanical ſkill of the workmen. According to this diviſion I ſhall endeavour to point out the very various effects which diſproportion in any caſe produces, and *vice verſa.*

In the preceding papers the coal and iron-ſtone have been traced through their various ſtages of preparation, and that ſtage pointed out in which they were moſt ſuitable for the profitable manufacture of the metal. It will be neceſſary to carry along with us this fact, that in the exact proportion which the quantity of carbon bears to the quantity of metal in the ore, and its mixtures, ſo will be the fuſibility, and of courſe the value of the pig-iron obtained. The importance of this truth will ſtill farther appear when we conſider the very various qualities of pit-coal, the different proportions of carbon which they contain, and the various properties attached to every ſpecies of this uſeful combuſtible.

Among the many ſtrata of coal which I have diſtilled, ſome I have found to contain 70 parts in the 100. This large proportion is peculiar to the clod-coal, uſed at ſome of the iron-works in England, and juſtly preferred, for the purpoſe of manufacture, to the pureſt and hardeſt variety of ſplint-coal. The latter I have found to average from 50 to 59 parts of carbon in the 100; and the ſoft, or mixed qualities of coal, from 45 to 53 parts. Such various proportions of carbon plainly point out, that the operations to be followed at each individual iron-work ought not to reſt upon

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precedent, unlefs borrowed from thofe works where exactly the fame quality of coal is ufed. This analyfis alfo lays open part of the fource from whence originates the widely different quantities of metal produced *per* week at various blaft-furnaces, and the great difproportions of ore ufed to different coals.

Experience has fhewn that the three qualities of coal juft mentioned, will fmelt and give carbonation to the following proportions of the fame fpecies of torrefied iron-ftone:—

112 lb. of clod-coal cokes will fmelt - -	130 lb.
112 lb. of fplint-coal cokes will fmelt - -	105 lb.
112 lb. mixed foft and hard coal cokes will fmelt	84 lb.

Let the iron-ftone be fuppofed in the blaft-furnace to yield 40 *per cent.* then we find that the 1-20th of a ton of the refpective qualities of cokes will fmelt and carbonate the following proportions of iron, *viz.*—112 lb. clod-coal cokes, 130 lb. iron-ftone, at 40 *per cent.* = 52 lb. iron; 112 lb. of fplint-coal cokes, 105 lb. of the ftone = 42 lb. of iron; and 112 lb. foft and hard coal cokes, 84 lb. of the iron-ftone = $33\frac{6}{10}$ lb. of iron. We then have for the quantity of metal produced by one ton of each quality of cokes:

Clod-coal	52	× 20 =	1040 lb.
Splint ditto	42	× 20 =	840 lb.
Mixed ditto	$33\frac{6}{10}$	× 20 =	702 lb.

This furnifhes a datum whereby we eafily obtain the quantity of the various cokes neceffary to produce 1 ton of carbonated crude iron by common proportion: for if 1040 lb of metal are produced by 1 ton, or 2240 lb. of clod-coal cokes, the quantity of the fame cokes requifite for the production of 1 ton, or 2240 lb. of metal, will be:

		T.	C.	Q.	lb.
[illegible]	4824·6 lb. =	2	3	0	8
Splint-coal cokes	840 : 2240 :: 2240 : 5973·3 lb. =	2	13	1	9
Mixed ditto	702 : 2240 :: 2240 : 7147·5 lb. =	3	3	3	7

If to the quantity of cokes neceffary to manufacture 1 ton of crude iron, we add the quantity of volatile matter driven off in the procefs of charring, which may be thus eftimated upon the average of each quality:—

Clod coal	$\frac{3}{8}$ or $37\frac{1}{2}$ *per c.*	produce in cokes	$\frac{5}{8}$	*a* $62\frac{1}{2}$ *per c.*
Splint coal	$\frac{4}{8}$ — 50	———————	$\frac{4}{8}$	or 50
Mixed coal	$\frac{5}{8}$ — 62,5	———————	$\frac{3}{8}$	— $37\frac{1}{2}$

Then,

Then, for the quantity of the respective coals used in the raw state, we have the following results in proportion:—

			T.	C.	Q.	lb.
Clod-coal	$5 : 4824{\cdot}6 :: 8 : 7719\frac{3}{4}$	=	3	8	2	19
Splint-coal	$4 : 5973{\cdot}3 :: 8 : 11946$	=	5	6	2	18
Mixed coal	$3 : 7147{\cdot}1 :: 8 : 19158\frac{2}{3}$	=	8	11	0	16

These great disproportions of quantity, used to fabricate 1 ton, or 2240 averdupoise pounds of the same quality of crude iron, will convey a striking and impressive idea of the multifarious qualities of coal which may be applied and made to produce the same effects. It should also convince the manufacturer that the study and analysis of his own materials is the first and radical approach to true knowledge, and certainty of operation. Divest him of this knowledge, and view him guided by the *customs* and *rules* prevalent at another manufactory, where the coals and ores may be as different as has been already mentioned, and we will no longer wonder at the uncertainty of his results, and the numberless errors of his direction.

Before I enter into the practical discussion of the application of coal, I beg leave to indulge myself in the following calculations:—We have already seen that the production of 2240 lb. of carbonated crude iron requires 4824 lb. of clod-coal cokes; these may be averaged to contain 4·5 *per cent.* of ashes, which, deducted from 4824, gives 4607 lb. of carbon used for 1 ton of metal: this sum, divided by 2240, farther gives, for 1 lb. of cast iron thus manufactured, 2·056 lb. of carbon.

We next find that 2240 lb. of the same metal requires of splint-coal cokes 5973·3 lb.; we farther find, from a table of the analysis of coal, furnished in a former paper, that 100 parts of the raw coal contained 4·2 parts of ashes. As it is there stated to lose 50 *per cent.* in charring, 100 parts of cokes will contain 8·4 of ashes; and 8·4 *per cent.* deducted from 5973·3, gives 5472 lb. of carbon. This again, reduced by 2240 lb. gives for each lb. of metal manufactured, 2·442 lb.

Again, 7147·1 lb. of cokes produced from soft mixed coals are consumed for every ton of 2240 averdupoise pounds of crude iron produced; every 100 parts of the same coals contain

tain 3·3 parts of ashes; and 100 parts of cokes contain nearly 6·5 *per cent.* of ashes, which, deducted from 7147·3, gives 6672·6 of carbon, which divided by 2240, gives, for the quantity used for 1 lb. of cast iron, 2·978 lb.

From these calculations it appears, that 2240 lb. of carbonated iron, requires of carbon from clod-coal 4607 lb.; of carbon from splint-coal, 5472 lb.; and of carbon from mixed coal, 6672 lb.: that 1 lb. of carbonated iron requires of carbon from clod-coal cokes 2,056 lb.; from splint, 2,442 lb.; from mixed, 2,983 lb.: and that carbonated crude iron may be obtained when widely different quantities of carbon have been consumed.

In seeking for a solution of the latter fact, we must have recourse to the different degrees of inflammability of the carbon, according to the various laws of continuity imposed upon it in its fossil construction. It can easily be conceived, that, owing to this structure, and the nature of the interposed ashes, the particles of carbon of some cokes will be more easily oxygenated than those of others; in the same way that we find splint-coal, when exposed to ignition in contact with open air, affords 1-3d of more cokes than are obtained from soft mixed coals, though the latter, when distilled, yields more pure carbon than the former.

By experiment it is proven, that 100 grains of carbonic acid gas is composed of 72 parts of oxygen united with 28 parts of carbon: if the quantity of the carbon of clod-coal, *viz.* 2·056 lb. used for the manufacturing of every pound of cast iron, is reduced to grains, we will find it to consist of 14392 grains; this, divided by 28, gives the acidifiable principle of 514 × 100 = 51400 grains of carbonic acid gas *:

* This is supposing, for the moment, that the whole of the carbon is oxygenated, either by the oxygen contained in the ore, or obtained from the discharging-pipe by the decomposition of the atmospheric air: this, however, is not strictly true, as the metal takes up a small portion, by weight, of the carbon; and when, by accident, moisture has been introduced into the furnace, either through the medium of the blast, or of the materials, its decomposition furnishes a portion of both oxygen and hydrogen, which may dissolve, and also carry off, a part of the carbon. Atmospheric air being found to hold water in solution, a small quantity of hydrogen will, even in the driest weather, be present in the blast-furnace. M.

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hence, as 1 cubic foot of this gas, at 29·84 of barometrical preſſure, and 54·5 of temperature, weighs nearly 761 grains, we find, that in the formation of every pound of caſt iron $\frac{51400}{761} = 67{,}54$ cubical feet of carbonic acid gas will be formed; and in the production of 1 ton of metal, the aſtoniſhing quantity of 151289,60 cubic feet. This quantity, however incredible it may ſeem, is only what would be formed under the above preſſure, and at the above temperature: when we take into the account the high temperature at which the decompoſition and recombination are effected, with the conſequent increaſe of elaſtic force and of volume, our ideas are almoſt unable to commenſurate the ſum of the gas hourly formed, and thrown off, ignited to the higheſt degree of heat.

If the ſame mode of calculation is adopted with the other qualities of coal, we will have the following reſults:—

For the ſplint-coal 2,442 lb. or $\frac{17094}{28} = 610{,}5 \times 100 = 61050$ grains of carbonic acid, which gives $\frac{61050}{761} = 82{,}85$ cub. feet for 1 lb., and $82{,}85 \times 2240 = 185{,}584$ cub. feet for 1 ton. For the mixed coal 2,983 or $\frac{20881}{28} = 710 \times 100 = 71000$ grains carbonic acid; that is, $\frac{71000}{761} = 93{,}3$ cubical feet for 1 lb.; and $93{,}3 \times 2240 = 208{,}992$ cubical feet for 1 ton. By the ſame calculation we may attain a pretty accurate notion of the quantity of atmoſpheric air neceſſary to produce 1 lb. or 1 ton of caſt iron; an average of the three varieties of coal will be ſufficiently accurate for this this purpoſe; thus $\frac{14392 + 17094 + 20881}{3} = 17455\frac{2}{3}$ or 2,4935 lb. of carbon are conſumed upon the average of each pound of pig-iron: this is found to produce of carbonic acid gas $\frac{17455\frac{2}{3}}{28} = 62{\cdot}341 \times 100 = 62{\cdot}30041$ grains; which again divided by 761, the grains in one cubic foot gives 81·86 cubic feet for the gas diſcharged in manufacturing one pound

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of caſt iron. As carbonic acid contains, as has already been noticed, 72 parts of oxygen in 100, then we have for the quantity of oxygen gas 100 : 72 : : 62300·41 : 44856·29 grains oxygen gas; and as, at the ordinary temperature and preſſure of the atmoſphere, a cubic foot of oxygen gas weighs 591 grains, we find 44856·29 divided by 591 = 75·89 cubic feet of oxygen gas neceſſary to form the acidifying principle of 81·86 cubic feet of carbonic acid gas; and that the ſame quantity of oxygen gas is neceſſary to the production of one pound of carbonated crude iron. This leads us to the following ſtatement for the quantity of atmoſpheric air uſed during the ſame operation; firſt premiſing that the conſtituent parts of atmoſpheric air are nearly 73 of azote and 27 of oxygen gas; of atmoſpheric air then neceſſary, we have 27 : 100 : : 75·89 : 281 cubic feet.

I ſhall now proceed from mere calculation to matter of fact, and attempt to prove the correctneſs of the former by the approximation of the latter to its reſults. Let a blaſt-furnace be ſuppoſed to produce $20\frac{1}{4}$ tons of pig-iron *per* week, = 45360 averdupoiſe pounds; this, divided by days, hours, minutes, and ſeconds, gives *per* day 6480 pounds, *per* hour 270, *per* minute $4\frac{1}{2}$ lb., and *per* ſecond 525 grains.

From this it is evident that 1 lb. of caſt iron is produced in $13\frac{33}{100}$ ſeconds: experience has ſhewn that a blaſt-furnace, producing, in any of the above periods, the reſpective quantity of metal, requires a diſcharge of air *per* minute nearly equal to 1350 cubic feet; this, divided by 4,5 lb., the quantity produced *per* minute gives, for 1 lb. iron, 300 cubic feet. The quantity, by calculation, we have ſeen to be 281 cubic feet—difference 19: a ſum no way conſiderable when we reflect upon the inequality of the movements of a blowing machine, and when it is recollected that ſome allowance ought alſo to be made for what air may paſs through the furnace undecompoſed, or may be loſt at the place of entrance.

From this coincidence of theory with practice, we cannot help admiring the rigorous principles on which the Lavoiſerian ſyſtem is founded; nor are we leſs pleaſed to find, that, ſmall as the operations of the chemiſt may be, yet they are a

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juſt epitome of what takes place in the philoſophy of extenſive manufactories. The following table exhibits the quantity of carbon which may be uſed upon an average, with the relative quantity of carbonic acid formed, and air uſed:—

In the manufacture of	1 lb.	—	1 ton of iron,
The pure carbon requiſite is	2·49	—	5585·44 lb.
Carbonic acid formed -	81·86	—	183366·40 cub. feet
Oxygen gas uſed - -	75·89	—	169993·60 cub. feet
Atmoſpheric air employed	281·00	—	629440·00 cub. feet.

From the foregoing particulars upon coal may be learned how much is dependent upon the native conſtruction of coal and its conſtituent parts; I ſhall next advert to the effects produced by its improper preparation.

When coals intended for the blaſt-furnace are ſufficiently charred, they ought, in point of colour, to be of a ſilver grey; their fracture will appear lamellated and porous if ſplint-coals have been uſed; ſofter coals form themſelves into branches ſlightly curved, and, when properly prepared, are always very porous. I have frequently found that the better the cokes were charred, the more water they will abſorb. Coals half burnt do not take up half ſo much water, becauſe their fracture continues in part to be ſmooth and leſs porous than when thoroughly burnt.

When half-prepared cokes are introduced into the furnace, the metal formerly carbonated will loſe its grey fracture, and approach to the quality of oxygenated iron. Their preſence is eaſily detected by the unuſual quantity of thick vapour ariſing along with the flame. Beſides, the water and ſulphur, which raw coals introduce into the furnace, and which always impair the quantity of carbon by the various ſolutions effected by the preſence of oxygen, hydrogen, &c. the fitneſs of the coal for combuſtion, and the ſupport of the ore, is much diminiſhed by this ſecond courſe of ignition and diſengagement of bitumen. The preſſure of the incumbent ores alſo fracture, and reduce the cokes into ſmall pieces, which produce a conſiderable portion of coke-duſt; this is partly carried to the top of the furnace before the blaſt: ſometimes below it appears in immenſe quantities, ignited to whiteneſs, and liquid as ſand. Coal thus detached from the maſs, ex-

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poſed to the action of a compreſſed current of air, is unfit for conveying the carbonic principle to the metal; and as it frequently belongs to the juſt proportion of charcoal neceſſary to ſmelt the ores, and to carbonate their iron, its loſs muſt be felt, and the quality of iron impaired.

When cokes of any quality are expoſed to a moiſt atmoſpheric, ſo as to abſorb water, their effects in the blaſt-furnace become much reduced, and the preſence of the water is productive of the moſt hurtful conſequences in the production of carbonated crude iron. I have found, by repeated experiment, that 1 lb. of well-prepared cokes will, when laid in water, take up $1\frac{3}{4}$ ounces in the ſpace of half an hour; at this rate, a baſket of cokes weighing 80 lb. ſaturated with water, will contain 140 ounces of water, or $8\frac{3}{4}$ lb. If the charge contains ſix baſkets, then we ſee that upwards of 50 lb. of water is introduced regularly along with the charge, furniſhing an additional quantity of oxygen equal to $42\frac{1}{2}$ lb., and of hydrogen equal to $7\frac{1}{2}$ lb.: it frequently happens that the cokes contain a larger portion of water than is here ſtated. When cokes thus ſurcharged are introduced in quantity into the blaſt-furnace, the quality of the metal is not always inſtantaneouſly changed, and frequently the colour and form of the cinder remain long without any great alteration. The contact of wetted cokes with the ore is firſt ſeen by the great diſcharge of pale-blue gas, with the whiter flame at the top of the furnace; next, the accumulating oxyde upon the ſurface of the pig when conſolidating indicates their preſence. Iron thus oxygenated frequently exhibits, while fluid, that agitation and delicate partings peculiar to carbonated metal: the remelting of this iron is never attended with advantage, and is always unprofitable to the founder.

From the properties aſſigned to pit-coal in this and in former papers, the following facts may be deduced:—That charcoal is the baſis of the manufacture of crude iron; that its proper application produces the moſt valuable qualities of pig-iron; that, by diminiſhing its relative proportion, or contaminating its quality by heterogeneous mixtures, the value and fuſibility of the metal is loſt; but that, by a proper increaſe,

creafe, and always in proportion to this increafe will the fufibility and value of the iron be mended. From the whole an important leffon may be learned of the pernicious effects of water in the furnace, and how abfolutely neceffary it is to prepare the cokes without ufing water, either to damp the fires, as in the ufual mode, or to cool the cinders obtained from the tar kilns, to prevent their confuming in the open air: in all this hurtful operation confiderable quantities of water become fixed in the cokes, which require a very great degree of heat to expel.

THE preparation of iron-ftone has already been fully attended to, and the phenomena which it exhibits under every ftage minutely defcribed. In confequence of various experiments we are authorifed to draw the following conclufions: That when pure calcareous iron-ftone is ufed, it admits of having the local quantity of cokes diminifhed; that argillaceous requires a larger portion than the calcareous genus; and that filiceous iron-ftone requires a greater proportion of fuel than any variety of the former genera. We have alfo feen that fufibility, either connected with ftrength or otherwife, is derived from the mixture of the ores; and that exceffive brittlenefs, intimately connected with infufibility, is alfo derived from the fame fource. From a review of thefe facts, we are forcibly impreffed with the importance of combining the prepared iron-ftones with proportions of fuel fuited to their various natures, in order to produce all the varieties of crude iron with the greateft poffible œconomy. Contemplating farther the fame fubject, it is eafy to be conceived that a want of knowledge of the component parts of iron-ftones, and the effects which individually they produce, muft lead to great uncertainty of operation in the fmelting procefs, wherein the beautiful œconomy of nature, and even real property, will be often unprofitably facrificed to precedent.

Befides the above caufes of alteration, dependent upon mixtures of the earths, the exiftence of oxygen in various quantities in the ores ought never to be overlooked in proportioning the cokes to the iron-ftone. This powerful agent, whofe

whofe form and fubftance conftantly eludes our vifion; whofe exiftence is only afcertained by the wonderful changes produced by its various combinations with the iron; and whofe prefence in the fame iron-ftone, in various quantities, may produce fuch variety of refult as to characterife the ores, as containing *good* or *bad* iron, furely forms the moft interefting mixture which ores or iron-ftones poffefs. It will be a momentous epoch in the manufacture of iron when the exiftence of fuch a principle fhall be fully admitted by the manufacturer, and its agency, from certain vifible effects produced, adopted to explain its accompanying phenomena. Till that period he will not perceive the utility of afcertaining the quantity of oxygen, and devifing œconomical methods of taking it from the ore. An attention to this powerful principle can alone root out thofe prejudices fo inimical to the real interefts of the manufacturer, and which feem to glance at Nature, as having improvidently combined her moft ufeful metal with mixtures which could refift the ingenuity of man, or fet his comprehenfive intellect at defiance. In the progrefs of this great inquiry, is it not poffible that the prefent expenfive exertions may in part be fuperfeded? Is it not poffible, that, by laying open the fources of information to individuals at large, a greater mafs of intellect may engage in the practice of this art? While the prefent extenfive and lofty buildings are neceffary, the bufinefs is entirely confined in the hands of men of great capital: the extent of their manufactures require that a large tract of country be devoted to their fupply; a natural confequence is, that innumerable fmall tracts of land are overlooked, or held unworthy of notice, merely becaufe they cannot, in a period neceffary to clear a great capital and infure a fortune, afford the neceffary fupply of materials. Such fituations, according to the prefent ftate of the iron bufinefs, muft remain unexplored. Should, however, a defire for truth once gain footing in the manufactories of iron, and fhould this natural impulfe of the unprejudiced mind keep pace with other branches of intellectual information, we may not defpair of feeing many imperfections removed, which were the unavoidable confequence of the period of their creation.

In

In the application of iron-ſtone in the blaſt-furnace, the following particulars ought rigorouſly to be attended to:—

1. Their mixtures, whether clay, lime, or ſilex: their relative proportions to each other, judging according to the rules formerly laid down; which of them may admit of a diminution of fuel; which of them will afford the quality of iron at the time requiſite; and which of them will be moſt likely, by a judicious arrangement, to give the greateſt produce of metal, united with value and œconomy. Iron-ſtones, united with large portions of ſilex, have already been ſtated to require a greater proportion of fuel to carbonate their metal than the other genera. When ballaſt or forge-pigs are wanted, it ſtands to reaſon that ſiliceous iron-ſtones ought to be uſed; not that they contain a greater quantity of iron, but becauſe they form a ſubſtitute for the other kinds, which may be more advantageouſly ſmelted for the production of more valuable qualities.

2. The quantity of metal which each individual iron-ſtone may contain, is another object of conſideration. Beſides the proportion of mixtures, which chiefly contribute to the fuſibility of iron-ſtones, a ſecond degree of fuſibility is dependent upon the richneſs of the ore in iron: this is ſo obvious in the uſe of the Cumberland and Lancaſhire ores, that the conſequences of their introduction will be perceived, by the change of the ſcoria and metal, in half the time that change would be effected by ordinary iron-ſtones. It has been frequently noticed, that crude iron contained pure carbon in proportion to its fuſibility; then the more fuſible, or ſupercarbonated qualities, muſt take up, comparatively, a conſiderable portion of the carbonaceous principle from the fuel. From this reſults a ſtriking conſequence, that the quantity of fuel ſhould, over and above its relation to the mixtures, bear a juſt proportion to the quantity of iron in the ſtone: for example, let the weight *per* charge of fuel at a blaſt-furnace be 400 lb., and let this be ſuppoſed ſufficiently to fuſe and carbonate the iron contained in 360 lb. of iron-ſtone; let the quantity of metal be ſuppoſed 35 *per cent.* then the produce will be 126 lb. Should a change take place, and iron-ſtone richer in iron be applied, though the ſame by weight, and ſhould this iron-

ſtone

ftone yield of torrefied ftone 45 *per cent.* its produce will be 162 lb. or 40 lb. more than the former. As there exifts no greater proportion of carbon in the furnace, it is evident that the exifting quantity, being diftributed over nearly 1-3d of more metal, muft therefore be in more fparing quantity in the whole, and the value of the metal confequently reduced.

3. The weight of oxygen contained in iron-ftones is the next object of ferious confideration. I have already fhewn, from experiment, that our iron-ftones naturally contain from 9 to 14 *per cent.* of oxygen, which remains after torrefaction; it has alfo been fhewn, that this quantity of hurtful mixture may eafily be doubled by over-roafting or under-roafting the ftone; and that the bad effects entailed are in the ratio of its combination with the iron. From a review of the facts adduced on this fubject in various parts of my papers, its agency and effects will eafily be credited by men of fcience; its property of conftituting the acidifying bafe of all the acids readily explains the unalienable confequence of its prefence with acidifiable bafes. The effects are ftill more pernicious when the oxygen is furnifhed by the decompofition of water in raw iron-ftone; the hydrogen in this cafe fet free, alfo feizes a portion of the carbon; and thefe abftractions, united to that produced by the native portion of oxygen in the ftone, form an aggregate which frequently reduces the value of iron 40 *per cent.* So long as the principles of fcience are overlooked in the manipulations of the foundry and forge, the exiftence of fuch agents will be treated as chimeras of the philofopher and chemift, and the effects hourly produced by them induftrioufly attributed to caufes which in point of unity or confiftency will not bear the flighteft touch of inveftigation.

XI. *A new Theory, pointing out the Situation of the Magnetic Poles, and a Method of discovering the Longitude. By* P. R. Nugent, *Esq. formerly Surveyor-General of Lands for the Island of Cape Breton. Communicated by the Author.*

THE several interesting and important voyages, undertaken by order of his present Majesty, for making discoveries highly beneficial to mankind, particularly in what relates to geography and navigation; and the great desire and intention of deriving every possible benefit from the many observations which such extended voyages were designed to offer ample room and opportunity for making, became objects of peculiar concern and consequence. In addition, therefore, to the undoubted and acknowledged abilities of the respective commanders and officers appointed to such designations, the Commissioners of the Board of Longitude thought fit to appoint, for every voyage, one or more persons on whose scientific ability, fidelity, and diligence in assisting to make the respective observations, they could also rest satisfied. These gentlemen were therefore furnished with the completest instruments which the most scrupulous care, intention, and circumspection, together with a total disregard to expence, could be found to procure. Their report and testimony is therefore conclusive proof of the perfection or imperfection of the several instruments they had with them; their report of the azimuth compass, and dipping-needle or compass, is as follows:—

Of the Azimuth Compass.

The late ingenious and accurate the Hon. Captain Phipps, afterwards Lord Mulgrave, in his account of his Voyage towards the North Pole, p. 108, remarks as follows:—"The variation of the compass, always an interesting object to navigators and philosophers, became peculiarly so in this voyage, from the near approach to the Pole: many of the theories that had been proposed on this subject were to be brought to the test of observations made in high latitudes, from which alone

alone their fallacy or utility could be discovered; they of course engaged much of my attention, and gave me the fullest opportunity of experiencing, with regret, the many imperfections of what is called the azimuth compass. This instrument, though sufficiently accurate to enable us to observe the variation so as to enable us to steer the ship without any material error, with the precaution of always using the same compass by which it is taken, is far from being of such a construction as to give the variation with that degree of precision which should attend experiments on which a theory is to be founded, or by which it is to be tried. The observations taken in this voyage will fully evince this by their great variations from one another in short intervals of time; nor is this disagreement of successive observations peculiar to high latitudes, and to be attributed to a near approach to the Pole, as I found it take place even upon the English coast."

Mr. Wales, F. R. S. Master of the Royal Mathematical School in Christ's Hospital, Secretary to the Board of Longitude, &c. in page 49 of the Introduction to the Original Astronomical Observations made in the Course of a Voyage towards the South Pole, and round the World, in his Majesty's ships the Resolution and Adventure, in 1772, 1773, 1774, and 1775, by himself and Mr. William Bayley, now Master of the Royal Academy at Portsmouth, published by order of the Board of Longitude, says:

" I cannot pass over this article without making a remark or two on the irregularities which we found in the observations made with these instruments in the Channel of England: the extremes of the observed variations were from $19\frac{3}{4}$ to 25; and all the way from England to the Cape of Good Hope I frequently observed differences nearly as great without being any way able to account for them, the difference in situation being by no means sufficient. These irregularities continued after leaving the Cape," *i. e.* they continued throughout the voyage. But these irregularities are trifling compared with those

Of the Dipping Needles, or Compass.

These instruments were so imperfect in principle, and the observations made with them so uncertain, that, excepting

the obſervations of the magnetic inclination, made at anchor or on ſhore, (and even theſe, as any one may perceive, were far from being certain,) no manner of uſeful certitude or judgment could ariſe from them. Mr. Wales, in his deſcription of the dipping needle here ſpoken of, and made by Mr. Nairne agreeable to a plan of the Rev. Mr. Mitchell, F. R. S. obſerves, in page 50 of the Introduction aforeſaid:

" The principal defects in this conſtruction are, the difficulty in placing the wires which carry the two laſt mentioned balls in the proper plane, and the total impoſſibility of knowing when they are ſo: moreover, it is very poſſible, and undoubtedly often happens, that the axis of the needle and its two poles do not lie in the ſame plane, in which caſe another difficulty will ariſe in adjuſting the needle to great exactneſs." And in page 15 of the Obſervations themſelves, Mr. Wales remarks as follows:

" The dipping needle which we took aſhore at this place (the Cape of Good Hope) was ſo much out of balance, and ſo difficult to get in again, that notwithſtanding we both of us (Mr. Wales and Mr. Bayley) ſpent all the leiſure time we had from other obſervations, we did not get it perfectly adjuſted before we went away, and of courſe were not able to get any obſervations of this kind at this time."

Mr. Bayley, in page 217 of the Original Obſervations made in the courſe of a voyage to the Northern Pacific Ocean, for the diſcovery of a north-eaſt or north-weſt paſſage, in his Majeſty's ſhips the Reſolution and Diſcovery, in the years 1776, 1777, 1778, 1779, and 1780, after, as appears, much precaution on obſerving with, and changing the poles of the dipping needle, &c. ſays: On the 28th July is mentioned an accident happening to the dipping needle: " the poles were not then changed, as the obſervations afterwards appeared regular; neither were the poles changed on receiving it on board, as it was ſaid to be well balanced: but the not doing it at either of theſe times was a great overſight, as we do not know the error of the dips with the marked end north, and dipping in any obſervations before to-day, and therefore cannot correct them according to Mr. Cavendiſh's directions. If we ſuppoſe the ſame error from the firſt as on to-day, (the mean

mean dip with the marked end north, exceeding the mean dip with the marked end south by 8° 8′), then are the mean dips from the beginning to be diminished by $\frac{488}{2}$ or 244 × cosine of the dips. If, again, we suppose no error on receiving it, nor from the above accident, the mean dips to this day will be lessened by $\frac{244}{2}$ or 122 × cosine of the dip."

What error or difference arose between the extreme of the observed dips with the marked end north, and those with the marked end south, are no where set down; they must, however, undoubtedly have been greater, and in all likelihood much greater than 8° 8′, seeing the mean result of the whole, small and great, gave 8° 8′. The observations made by Lord Mulgrave are of the like nature and uncertainty.

The whole of these instruments were therefore totally inadequate to the correct and useful purposes of navigation, or indeed to any correctly useful purpose whatever: they were however, as before observed, the very best that could be obtained; and in the making of which no expence whatever on the part of the Board of Longitude, nor pains on the part of those who made them, were wanting to render them as perfect as possible, and such as should answer the purposes for which they were designed; nor do I entertain any doubt they were as much so as the nature of their construction could possibly admit of.

My pursuits in life having been considerably connected with objects of this kind, my own experience, corroborated by the above and like testimonies, together with the continual necessity, which arose from time to time, of increasing the magnetic orbit so as to correspond to the increase of the variation (on the supposition that the true dip and variation might be accounted for and predicted,) by means of moveable magnetic poles *, led me into endeavours to investigate

* William Whiston, M. A. in the 8th page of an historical preface to a pamphlet of his, entitled, "The Longitude and Latitude found by the Inclinatory or Dipping Needle," gives us the brief history of all theories then published, in these words, speaking of the magnetic poles and theories: "However, this notion in general of moveable magnetic poles has always

tigate fome theory more conformable to the laws of Nature *, and which would reconcile the continual difagreement between men of fcience on this fubject: and this defire alfo of neceffity led me into endeavours to form more perfect nautical inftruments, for without more accurate obfervations no effential utility whatever could arife to navigation from any kind of theory however perfect. To detail the train of reafon-

ways feemed fo probable, or rather neceffary, ever fince the variation has been itfelf found to vary, and this after a certain regular manner; alfo, that Mr. Phillips before Mr. Bond afferted the fame, and ftated the revolution to be in 370 years; and after all, our learned Dr. Halley, who has far outdone every body upon this fubject, has determined it to be fo, only he has thought himfelf obliged to add the hypothefis of two other fixed poles; and from the joint effects of all four poles, and from thofe only, has he been able to bring this variation of the variation to fome kind of fyftem agreeable to the obfervations. He has alfo been obliged to lengthen the period of the moveable poles' revolution; and as Mr. Bond had enlarged Mr. Phillips's number from 370 years to 600, fo has Dr Halley enlarged the fame farther, from 600 to 700 years;" and Mr. Whifton, in page 58 of the pamphlet itfelf, ftates the revolution of Dr. Halley's inner nucleus, or north magnetic pole, to be in 1920 years, which, he obferves, is much flower than that of Mr. Phillips, Mr. Bond, or Dr. Halley.

Since Whifton's time, thofe who attempted the magnetic theories are: the celebrated Euler, who, as all others have hitherto done, fuppofes that the magnetic poles move; Mr. Lorimer, Mr. Churchman, Mr. Walker, &c.; but of all thefe, Mr. Churchman is the only perfon that determines their revolution, which he ftates to be, the northernmoft in 1096 years, and the fouthernmoft in 2289 years.

I fhall forward, for a future number of the Philofophical Magazine, a more full account of the theories that have hitherto prevailed, in which I fhall enter more fully, than my prefent limits will allow, into the truth of my new theory, and of the method by which I have been enabled to prove the fallacy of the former. In the mean time any perfon, even by roughly perufing the obfervation of latitudes, longitudes, and dips, made on the north-weft coaft of America, and comparing them with that at London, and alfo with thofe made off the eaft coaft of South America and in the Chinefe feas, will foon be enabled to afcertain, fufficiently near for the purpofe of remaining fatisfied, that the fituation of the magnetic poles is as defcribed: my determinations are, however, drawn from a multitude of calculations, and the refult compared with all the magnetic dips and variations (taken in all the different places on fhore) I could procure.

* I account for the change of the magnetic variation by *original magnetic meridians* and a *magnetic annulus.* N.

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ing and consequent investigation I went through, would be unnecessary here. The result, which I have long since explained to many individuals, I now, in as brief a manner as the subject will admit of, lay before the public, not doubting that an object, which actually tends to the enlargement and perfection of science, and, in my opinion, to the benefit of navigation, will find favourable reception, investigation, and experiment.

Method of discovering the Longitude, the Magnetic Inclination and Latitude of the Ship being given:

PROBLEM the First, being the THEORY.

To determine the Latitude and Longitude of the Magnetic Poles, and, in consequence, for the purpose of discovering the Longitude, to ascertain a First Meridian.

Let N (Plate VIII.) represent the north pole of the earth, M the magnetic north pole, S Smeerenberg harbour in Spitsbergen, and L London: also, let arches of great circles be drawn from London to the north magnetic pole, to the north pole of the earth, and to Smeerenberg, and also from Smeerenberg to the north magnetic pole. There will thus become formed three spherical triangles, which are resolved as follows:—

1st, In the triangle NLS are given the sides NL and NS, being the co-latitudes of London and Smeerenberg, and the included angle LNS being their difference of longitude, to determine the angle of position NLS and the third side LS.

2d, In the triangle LSM are given the three sides LS, (just found) LM and SM (LM and SM being the magnetic polar distances corresponding to the correct dips at London and Smeerenberg), to determine the angle MLS, from which subtracting the angle NLS lately found, there remains the angle of position NLM.

Lastly, In the triangle MLN are now given the sides LM and LN, and the included angle MLN, to find the angle LNM, the longitude of the north magnetic pole (and the meridian of the earth passing through the magnetic poles and

and poles of the earth,) from the meridian of London, and the fide M N the diftance of the magnetic pole from the pole of the earth.

The place of the magnetic poles being thus obtained, the queftion next divides itfelf into two points of view, namely, Whether the magnetic poles or points thus determined are ftationary or otherwife? the folution of which depends on obfervation and philofophic inference. From obfervation it is remarked, in page 121 of the late Lord Mulgrave's Account of his Voyage towards the North Pole, that—

"There is no reafon at prefent to fuppofe that the dip is liable to any variation in the fame place at different periods of time. It having been obferved at London by Norman, who firft difcovered it in 1592, to be 71°.50′, and by Mr. Nairne, in 1772, about 72°: the difference between thefe obfervations, taken at fuch diftant periods of time, is fmaller than that found between feveral of Mr. Nairne's obfervations compared with each other; and therefore we have no reafon to conclude that the dip has altered fince Norman's time. The care with which his inftrument was conftructed, and his obfervations made, leave us no room to doubt of their accuracy."

Again, Mr. Cavallo, in his Treatife on Magnetifm, p. 65, remarks, (fpeaking of the dip,) "Its alteration in the fame place at different times is very fmall; thus in London about the year 1576, the north pole of the dipping needle ftood 71.50 below the horizon, and in the year 1775 it ftood at 72.03: the alteration of the inclination in fo many years amounting to lefs than a quarter of a degree, which may be attributed to the error of the inftruments, fince, as will appear in the fequel, thofe inftruments are far from having attained to any degree of perfection even in the prefent age."

Now, from philofophic inference it will naturally follow, that if the inclination of the magnetic needle at different periods of time at any one place remain conftant, the magnetic poles are ftationary; and if the magnetic poles are ftationary, (as appears clearly evident from above,) the meridian thus difcovered is the *Firft* (or only) *Meridian* from which the longitude

gitude ought to be counted; I have always used it as such, and obtained the most satisfactory results.

PROBLEM the Second, being the PRACTICE.

To determine the Longitude universally.

Here are always given the ship's polar distance, the magnetic polar distance, and the magnetic co-latitude, being three sides of a spherical triangle, to determine the angle at the pole of the earth opposite the magnetic co-latitude, being at all times and places the longitude of the ship east or west (as the case may be) from the meridian aforesaid.

N. B. The magnetic co-latitude is the distance of the ship from the nearest magnetic pole; the ship's polar distance is the distance of the ship from the pole of the earth next adjoining the said magnetic pole; the polar distance is the distance between either magnetic pole and the pole of the earth nearest thereto: moreover, the longitude thus obtained can never exceed 180. It may, however, be easily reduced as to that estimated from any assumed first meridian. Likewise the point M, thus assumed as the magnetic pole, may, to prevent perplexity, be better understood as being that point on the surface of the globe at which the dipping needle stands at right angles to the plane of the horizon, without regarding whether the magnetic poles themselves are under, at, or above the surface of the earth.

The method of obtaining and establishing (from observation alone) certain data for investigating or corroborating perfect tables of magnetic latitude and co-latitude, appears (from the preceding theory) obvious, being, by carefully making different dips of the magnetic needle along the meridian aforesaid, and carefully observing the respective latitudes at which those dips are taken, and for greater certainty (if thought needful) continuing them, as far as possible, into both hemispheres; though it will also be perceived they may be investigated by observing different dips, along any other great circle, passing through the magnetic poles, or having determined the precise latitude and longitude of the magnetic poles. Correct tables may also be deduced from correct dips at various places; however, the

firſt method is not only the moſt eaſy, but on many accounts the moſt preferable; and for this purpoſe the correct dips of the magnetic needle, taken at the following places, (regard alſo being had to determine their correct latitudes and longitudes, particularly the former, except near the magnetic poles, where it is neceſſary to correctly determine both,) will be found ſufficient, namely, at the Falkland Iſlands, Buenos Ayres, Oroonoque, Trinidad, Barbadoes, Saint Luke, Martinique, Guadaloupe, Antigua, Saint Martin's, Bermudas, the Atlantic Ocean, in or near the ſame longitude up to and near the Iſle of Sables, at the Iſland of Cape Breton, the ſouth ſide of the river Saint Lawrence in the longitude aforeſaid; then going round to the north-eaſt ſhore of the Labrador Coaſt, and obſerving thereon at Davis's Inlet, and ſo on up to the northward under the meridian aforeſaid, up to and into Baffin's Bay, and there carefully determining the latitude and longitude of the north magnetic pole; or firſt going into Baffin's Bay, and afterwards proceeding to the ſouthward.

Next, by carefully obſerving the magnetic inclination along the oppoſite meridian in Ruſſia, China, the Chineſe Seas, Indian Ocean, &c.; as at the mouth of the Leno, at Olikſminſkoi, at Nutſchink or Albazin, at Pekin, Nankin, Nimpo, Formoſa, the Philippine Iſles, Gindano, Celebes, Flores, the north and ſouth ſide of New Holland, and ſo continuing to obſerve along the Indian Ocean, in or near the meridian aforeſaid, up towards the ſouth pole of the earth, until the place of the ſouth magnetic pole be carefully determined, *i. e.* whatever pole may be firſt convenient to determine. The number of places herein ſet forth are mentioned in order that navigators and others, as opportunity may offer, may know where and how to make the advantageous obſervations herein alluded to.

It may alſo be proper to add, that hitherto the places of the magnetic poles have always been attempted to be found by means of the magnetic variation; a method whereby the moſt able mathematicians have been continually led aſtray, for, as the magnetic variation is continually changing, their concluſions

conclusions were rendered as uncertain as the variation itself, of which (it had so happened from the method they pursued) they knew little more than that it did change. Had the present theory occurred to them, it would have received chearful and favourable countenance and encouragement, and it and its utility been soon and effectually established. Had the indefatigable and famous Dr. Halley in particular known thereof when he undertook two voyages (long after the inclination of the magnetic needle was discovered, namely, in 1698 and 1699) for the express purpose of endeavouring to form a magnetic theory by means of a multitude of observations of the magnetic variation, as a preparatory step, and in order to determine the longitude thereby, or, as his instructions run, "to seek, by observation, the rule of the variation of the compass;" the observations of magnetic dip here recommended, and which he would have made at least along the western line before pointed out, together with the observations of the variation of the compass, which he did observe, in all likelihood, if made with instruments capable of affording the requisite accuracy, would have soon suggested, to his penetrating and fruitful genius, the correct object of his research.

It may be farther proper to remark, that the observations of the magnetic inclination herein stated, and made at London, do of themselves alone establish another important conclusion, likewise quite different from the general opinion of the Learned, which supposes that the magnetic variation affects (*i. e.* increases or diminishes) the magnetic inclination; for, from the time in which the magnetic inclination was first observed at London to the present time, the variation has shifted upwards of 36° without affecting the inclination; and therefore, after knowing the result of so very great a change in the magnetic variation without having affected the inclination, there can be no reason to conclude that it can in any respect be affected thereby: a circumstance that renders the theory here pointed out still more simple and beautiful.

Thus, by new and interesting application and testimony of unerring science, practical observation, and fair conclusion,

 I have,

I have, I flatter myfelf, cleared the magnetic theory of the abftrufe and embarraffing confiderations which have hitherto bewildered it, and thereby laid the foundation of a moft fimple, general, and ufeful method of determining a fhip's correct place at fea, without any regard to the fun, moon, or ftars, to good or bad weather, to the time of the day, the day itfelf, months, years, feafons, or centuries, except fo far as may be faid to regard the magnetic inclination, which muft always be known from obfervation; as alfo the latitude of the fhip, which muft likewife be known, either from obfervation, or dead reckoning.

I would not have it underftood, however, that I mean to decry the ufe of lunar obfervations or chronometers. Far otherwife: I have devoted great application to the ftudy of the lunar (and other) aftronomy; and herein I am happy to add, I have fucceeded to an accuracy and concifenefs of equations and calculations far furpaffing Mr. Mayer's, or any lunar tables yet offered to the public. But herein, notwithftanding the high efteem and veneration in which the great genius and labours of Euler, and the merits of Mr. Mayer muft ever be held by men of fcience, I found it neceffary to reject, for reafons which I fhall hereafter tranfmit for infertion in the Philofophical Magazine, the fuppofition that the motion of the fun or earth, according to Mr. Euler, and of the moon according to Mr. Mayer, are different now to what they formerly were. I am alfo the original inventor, and for which I have, for upwards of feven years, had a patent, of a double fextant for making lunar obfervations to the right and left. I have alfo invented a metal quadrant, by which, with fore-adjuftments and obfervation, the diftance of the moon can be meafured from the fun or other object, from one limit or extreme point of the horizon to the other.

From the multitude of lunar places which I have calculated from my tables, I am perfuaded they are fuch as fhall not, even at the equator, produce a mean error of five, nor an extreme error of fifteen miles in longitude. The firft of the inftruments here fpoken of (a double fextant) meafures with convenience, without inverting the inftrument, the fimple limits of a fextant on either fide the fun or a fixed ftar:

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to the other there can be no limits, for it meafures 180 degrees on either fide. Not fatisfied with doing thus much, in order to avoid the inconvenience, trouble, and inaccuracy arifing from the ufe of large volumes of charts of fpherical triangles, and of tables for reducing obferved lunar diftances to true, I contrived an inftrument, for which, as well as feveral fuperior furveying and other inftruments, I have alfo got a patent. This inftrument, which I have denominated a fuit of circles of calculation, performs this operation with eafe, accuracy, and expedition. It alfo determines the true latitude, without knowing the latitude by account, either by double altitudes and the time between, by double altitudes and the difference of magnetic azimuths, (which my inftruments will correctly give,) or by fimply having the altitude of two known fixed ftars. It determines the time of the day, &c. &c. In a word, it refolves all manner of fpherical triangles or fpherical trapezia, &c. The refolution of the few of thefe that conftitute the effentials of nautical aftronomy and geography, becomes by this means obvious, eafy, and pleafant; whereas the underftanding, or even the appearance of the previous burthen of requifites, creates, as is well known, a great if not infuperable obftacle to the generality of otherwife well-informed feamen.

Having thus, I truft, explained my regard and efteem for the lunar obfervations and chronometers, all I would have underftood of the method of difcovering the longitude by the dip is, that it is eafily underftood by the moft ordinary capacity; that to others, in every inftance, it becomes an auxiliary or corroborating proof, and, in the time of greateft neceffity and peril, a moft eftimable fubftitute; not requiring, in this cafe, any calculation whatever. Thus, from the preceding theory, the dip in all places remaining the fame, the navigator, being perhaps feveral days without feeing fun, moon, or ftars, fo as to make any ufe of either towards determining his longitude, has this benefit left him, that he can, at any moment of the day, in which the extremity of the horizon can be feen, determine his correct dip, and therefore his vicinity towards the land he wifhes to make or avoid; where otherwife he might either be caft away, or, under the

the ſuppoſition that he was near thereto, keep either lying-to, or ſtraining his ſhip in endeavouring to get no farther to leeward, when at the ſame time he might be an hundred miles or more from ſhore, and ſo loſe the very wind he wanted; after which, ſhould contrary winds aſſail him, the injury alſo becomes obvious.

To clear apparent or obſerved Lunar Diſtances from the Effects of Refraction and Parallax by the Suit of the Circles.

Set the vernier on the firſt meridian, carrying the circle or ſemicircle of poſition to the apparent altitude of the ſun's centre, or that of the ſtar; the vernier on the moveable meridian to the apparent altitude of the moon's centre; and the vernier on the circle or ſemicircle of poſition to the apparent diſtance of the ſun and moon's centres, or of the centre of the moon and fixed ſtar. Let theſe form a ſpherical triangle; ſo ſhall the number of degrees, minutes, and ſeconds on the equator, comprehended between the firſt and moveable meridian, ſhew the angle (or difference of azimuths) at the vertex, which angle is common to the apparent and true triangle. Keeping faſt, therefore, the ſaid meridians by means of the equator, ſet the vernier on the firſt meridian to the true altitude of the ſun or ſtar's centre, and the vernier on the moveable meridian to the true altitude of the moon's centre; then ſee what number of degrees, minutes, and ſeconds of the circle or ſemicircle of poſition or diſtance are contained between them, for that is the true diſtance.

Portſmouth, Dec. 13, 1795.

We certify, that, in our opinion, a compaſs and ſextant invented by Mr. Nugent merit an expeditious and accurate inveſtigation and trial.

(Signed) T. Packenham, *Le Juſte.*
J. Craunston, *Bellerophon,*
E. Gower, *Triumph.*
W. Domett, *Royal George.*
R. Bowen, *Terpſichore.*

The foregoing certificate was given to me, unſolicited, for the purpoſe of being tranſmitted to the Lords Commiſſioners of

of the Admiralty, in order to procure their Lordships order to have the merits and efficacy of my inventions and instruments officially examined into and reported upon. As to the instruments, they were on the same day applied for, through Commissioner Marsh, to the Navy Office, by the above gentlemen.

The most speedy and least expensive method of ascertaining the precise situation of the north magnetic pole, the southern being opposite thereto, appears to be, by engaging one of the Davis's Strait whalers to proceed into Baffin's Bay, with a proper person duly qualified, to make the necessary astronomical and magnetic observations; or rather, by dispatching for this purpose a frigate or other vessel in the king's service employed on the Newfoundland station. Pity it was, indeed, that the Board of Longitude had not known of this method, and of the reasons that render all navigation into the South Sea, by any passage to the eastward of Greenland, totally impracticable. The Hon. Captain Phipps's destination would in that case have been to proceed into Baffin's Bay, where, had he also possessed the angular and magnetic instruments herein alluded to, he would have assuredly ascertained one great object of his voyage, namely, the determining the fallacy or utility of all previous theories, and bringing them to the test of experiment; a test which would have caused the whole to have vanished, and none other would have remained but that I have already pointed out. Nor do I entertain much doubt that, by attending to circumstances, which might easily be pointed out, Captain Phipps would have also accomplished a passage into the South Sea.

The principles of the whole of my nautical magnetic instruments (except a variation frame for converting all steering into variation compasses) is the application of a quadrant, sextant, or double sextant, (fitted for this purpose,) to the vertical magnetic axis of the horizontal needle for determining the variation, and to the lateral magnetic inclinatory axis of the dipping needle for determining the dip. These needles are also differently suspended from others: the dipping needle, for example, is always at liberty to resort to, and remain

at

at reſt in, the common interſection of the lines of the magnetic meridian or variation and dip; nor can any alteration of direction or motion of the ſhip affect a dipping needle thus ſuſpended.

In conſequence of this ſimple contrivance, the motion or gyration of the graduated circles for either dip or variation on account of the motion of the ſhip, becomes totally avoided; for, on or in theſe magnetic inſtruments there are no diviſions whatever: the inſtruments themſelves can be afforded for half the price of others; the navigator makes his obſervations like a man of ſcience; and finally, he determines with expedition, eaſe, and certainty, the magnetic dip and variation, to degrees, minutes, and ſeconds, *i. e.* to the like accuracy that he can diſtinguiſh or read the ſubdiviſions ſhewn by the vernier on the arch of his ſextant: a circumſtance utterly impoſſible to be obtained even to whole degrees, it might be ſaid to half a dozen degrees, by any other conſtructed inſtruments, as has been herein before ſufficiently verified.

XII. *Deſcription of a Portable Machine for loading and unloading Goods.* By *Mr.* GEORGE DAVIS, *of Windſor, Berks* *.

THE ingenious contriver of this machine is certain, that, when made of its intended ſize, it will be capable of loading a ton weight by one man only, and will be ſo portable as not to exceed one hundred and twelve pounds in weight.

Reference to the Figure of the Machine. (See Plate IX.)

A, the winch, which turns the bar B. This bar has on it two endleſs ſcrews or worms C, C, which work in the toothed wheels D, D. Theſe wheels are fixed to the barrels E, E, round which the ropes F, F, coil, wind up, or let out the ſame occaſionally; which ropes, paſſing over the two

* From the *Tranſactions of the Society for the Encouragement of Arts, Manufactures, and Commerce.*

A premium of forty guineas was given to Mr. Davis for this invention.

pulleys

pulleys G, G, are brought round, and their ends, having hooks for that purpofe, are hitched into ftaples fixed to the front of the cart or other carriage: within thefe ropes the load H is placed on a common pulley I, which forms an inclined plane, up which, by the turning of the winch, the ropes are wound upon the barrels, and the load raifed into the carriage.

K K, the frame, intended to reprefent the part of the cart, or other carriage, on which the machine is occafionally to be placed.

The whole of the barrels and cogged wheels are contained in an iron box L, the fides of which are reprefented in the figure as taken off, that the conftruction of the feveral parts may be fhewn.

XIII. *Defcription of a cheap and efficacious Ventilator for preferving Corn on Ship-board.* *By* THOMAS SOUTH, *Efq.* *.

THE importation of grain is a precarious traffic. The produce of diftant countries, or even of thofe near home, when long in collecting, or long detained on fhip-board, is fubject to heat, foon becomes fetid, and is often fo far fpoiled and depreciated in its value as to fell for lefs than the original coft. Hence the merchant, overwhelmed with loffes, regrets his patriotifm, grows fhy of importation, and, unlefs invited by a certainty of gain, drops the trade, even whilft the nation ftands in need of fupplies.

The remedy here propofed is a fimple, cheap, and, I truft, efficacious method of ventilating grain whilft confined on fhip-board; fufficient, I prefume, to keep it fweet and marketable after fuftaining a tedious voyage.

Defcription of the Ventilator, with References to the Figures thereof. (See Plate VII.)

Fig. 1. is a cylindrical air-veffel or forcing-pump, of lead,

* From the *Letters and Papers of the Bath and Weft-of-England Society for the Encouragement of Agriculture, &c.*

tin, or other cheap metal; its internal diameter being ten inches, and its length three feet; having a crutch-handled piſton to work with, and an iron noſle, *viz.* a hollow inverted cone, two feet long, to condenſe the air, and increaſe its power in its paſſage downwards. This cylinder ſhould be riveted or ſcrewed, by means of an iron collar or ſtraps, to the deck it paſſes through, both above and below, as at *a a*; and ſhould be farther ſecured by ſome hold-faſt near *b*, to keep it ſteady in working.

Fig. 2. is a bottom of wood, four inches and a half thick, with a projecting rim at its baſe, for the metal cylinder to reſt on, when cemented and ſcrewed to the wood. The centre of this bottom is excavated, for the reception of the crown of the noſle. In the ſame figure the noſle is repreſented with its crown like a bowl-diſh, to condenſe the air gradually, without reſiſtance, in its advance to the more contracted baſe of the inverted cone, *i. e.* the top or entrance of the noſle. About two-thirds down this noſle may be fixed a male ſcrew, as *c c*, for the purpoſe hereafter mentioned.

N. B. The forcing-pump ſhould be caſed in wood, to protect it from outward bruiſes, which would prevent the working of the piſton, and ruin its effects. The leather round the embolus ſhould be greaſed when uſed.

Fig. 3. is a crutch-handle, faſtened to the embolus A by its iron legs B, B. A is a cylinder of wood, caſed with leather, ſo as to fit well, but glide ſmoothly in the metal cylinder; having an opening as large as its ſtrength will permit, for the free acceſs of atmoſpheric air. C is a valve, well leathered on its top, and yielding downwards to the preſſure of the air when the piſton is raiſed up. D is a croſs bar of iron, to confine the valve, ſo that it may cloſe inſtantly on the return of the piſton downwards.

Fig. 4. is a tin pipe or tube, of leſs than four inches diameter, and of ſuch length as, when fixed to the baſe of the cylinder, Fig. 1, ſhall admit the noſle *d*, Fig. 2, to within half an inch of the valve E, at the bottom of the wooden cylinder F, in Fig. 4; which valve E will then yield to the preſſure of air condenſed in its paſſage through the noſle, and deliver it into the pipes below. This valve muſt be well

leathered

leathered on its upper surface, and fastened with an hinge of leather to the cylinder it is meant to close: affixed to its bottom is the spindle G, passing through a spiral spring H, which, being compressed on the descent of the valve, will, by its elasticity, cause it to rise again, close the aperture above, and retain the air delivered beneath it. On connecting this cylinder with the upper end of the nosle, at *e e*, Fig. 2, we must carefully prevent any lapse of air that way, by a bandage of oakum smeared with wax, on which to screw the cylinder, like the joints of a flute, air-tight. I is a bar of iron, having a rising in its centre, wide enough for the spindle to play through, but at the same time sufficiently contracted to prevent the passage of the spiral spring.

Fig. 5. is an assemblage of tin pipes, of any lengths, shaped suitably and conveniently to their situation in the ship, to the form of which, when shut into one another, they must be adapted; observing only, that the neck be straight for a length sufficient to admit the lower end of the cylinder, Fig. 4. as high as the letter F, or higher.

Fig. 6. To the middle pipe, which runs along the bottom, should be fixed a perpendicular one, fully perforated, to convey the air more readily into the centre of the heap; and this may have a conical top, as represented in the plate, perforated with a smaller punch to prevent the air from escaping too hastily. In large cargoes, two or three of these perpendiculars may be necessary; and each should be well secured by an iron bar *g*, screwed down, to prevent their being injured by the shifting of the cargo in stormy weather or a rolling sea. The top of the conical cap of these pipes may reach two-thirds up the cargo.

Fig. 7. is a valve of the same construction as that represented in Fig. 4, but inclosed in a tube of brass, having a female screw at *f f*, adapted to the male screw *c c*, on the nosle Fig. 2, and may then be inserted into the head of the pipe Fig. 5. This will add to the expence; but, in a large apparatus, is to be preferred, as a more certain security from lapse of air, than the junction of the tube Fig. 4, to the neck *e e* in Fig. 2.

N. B. *c e* is a neck of wood, making a part of the bottom

Fig. 2, whereon to secure the tube Fig. 4, when applied to the nosle. The joints of the pipes, when put together for use, should be made air-tight, by means of bees-wax or some stronger cement, till they reach the bottom of the vessel, when there is no farther need of this precaution. The horizontal pipes should run by the side of the kelson the whole length of the hold. The tin plates of which K is made, should be punched in holes, like the rose of a watering-pot, in two or three lines only at most, and then formed into a tube, with the rough side outwards. L may have four or five lines of the like perforations. M, and the rest, should gradually increase in their number as they advance towards the middle of the hold, and continue fully perforated to the last pipe, which should be closed at its end to prevent the ingress of the corn. It is the centre of the cargo which most requires ventilating, yet air should pervade the whole. Like the trade-winds, it will direct its course to the part most heated, and, having effected its salutary purpose there, will disperse itself to refresh the mass.

Where the hatches are close-caulked, to prevent the influx of water, vent-holes may be bored in convenient parts of the deck, to be bunged up, and opened occasionally, from whence the state of the corn may be known by the effluvia which ascend when the ventilator is working.

The power of the ventilator is determined by the square of its diameter multiplied into the length of the stroke, and that again by the number of strokes in any given time.

To find the area of a circle, and the solidity of a cylinder raised on that circle, Archimedes gives the following proportion:—

As 1 is to .785398 decimal parts, so is the square of the diameter to the area of the circle.

And, as 1 is to .785398, so is the square of the diameter, multiplied by the height, to the solidity of the cylinder.

The cubical contents, both of cylinders and tubes, are found in the same manner; their difference consisting not in shape, but solidity, the latter being hollow.

Then, to find the contents of a cylindrical vessel whose internal diameter is ten inches, multiply that into itself, and the

the ſquare thus obtained, multiplied by .7854, will give the contents of the circle in cubic inches; which, multiplied again by twenty-four inches or lengths of the ſtroke, being the proportion of the barrel filled with air, gives in cubic inches the amount of each diſcharge on the deſcent of the piſton. As thus:

Inches.

Internal diameter of the pump or tube 10
× 10

= 100, or ſquare of the diameter;
which, multiplied by .7854, to bring the contents of the ſquare to the contents of the circle.

Which, multiplied by the length of the ſtroke, } 78,5400 Contents of the area of the circle.
24 inches, produces 1884 cubic inches.

3141600
1570800

18849600

which, divided by 231)1884.9600(8.1600 gallons, which is $\frac{16}{100}$ths more than 8 gallons at a ſtroke; allow theſe decimals for waſte of air in each ſtroke; and 60 ſtrokes to be made in a minute.
viz. the number of cubic inches in a wine gallon, quotes 8 galls.
369

1386

Then - - - - - - - 8 gallons diſcharged at a ſtroke,
multiplied by 60 the number of ſtrokes

amounts to 480 gallons *per* minute;
which multiplied by - - 60, the minutes in an hour, produces 28800 gallons in that time;

and that, divided by 252)28800(114.3 tons.
(the number of gallons in a ton, both wine and ſhip meaſure) quotes 114 tons in an hour.
.360

1080

..720

Then, ſuppoſe the area of the hold of a ſhip to be = 120 tons, and, when freighted, the interſtices between the grains, together with the area between the ſurface of the corn and the

the underfide of the deck = 5 tons = to the quantity of mephitic air confined; fuch being the lighteft fluid, the major part of it would, foon after the commencement of the operation, be forced, by the atmofpheric air, to vent itfelf at the holes provided for that purpofe; and the remainder of the hour being employed in the like ventilation, five tons of frefh air would pafs above twenty times repeatedly amidft the grains, to cool, refrefh, and fweeten the cargo. A purification thus adminiftered once in eight-and-forty hours, would, I conceive, be amply fufficient to preferve the corn from taint or injury, be the voyage ever fo tedious; and unlefs it fhould by neglect have overheated and grown together, or fettled too clofe, the labour would be that of a boy only; for the dairy-girl at her churn works harder than he otherwife need to do at this.

My air-veffel is, for the fake of cheapnefs, confined to the narrow diameter of ten inches; but, as the contents of circles are proportionate to the fquare of their diameters, by enlarging that, you increafe their power accordingly; wherefore, by extending the diameter to fourteen inches, the contents will be nearly doubled; and, by adding ten inches more to the length of the ftroke, you almoft treble the difcharge of No. 1, and obtain a power capable of ventilating a cargo of 400 tons within the hour. But the air-veffel muft be lengthened; the pipes at the fame time enlarged; the metal of which the whole is conftructed be in fubftance proportionable; and the labour be that of a man, or perhaps two upon occafion.

A ventilator, on the plan and dimenfions here propofed, would come within the compafs, I fhould think, of five or fix guineas. One on the larger fcale, caufed by the increafed fubftance of the metal, and the extra fize and length of the pipes, might amount to twenty; which, in either, is under fourpence *per* quarter on the firft cargo; and as they will laft many years if well painted, and, when not in ufe, taken to pieces and put carefully by, I flatter myfelf it is an experiment well worth trial; particularly if a premium be offered to the fhip-owner, who, by means of fuch machine, imports his corn pure and untainted from a diftant land.

Objections

Objections made to the fuppofed Effect of the Ventilator, over-ruled, it is hoped, by the Confiderations which follow them.

Firft, The holes pierced in the tin tubes which are to lie under the corn, feem capable of iffuing (efpecially if an effort be made upon them) a much larger quantity of air than the forcing-pump will fupply in a given time. Confequently, a given quantity of thefe holes, under a given preffure, will be capable of iffuing the whole fupply of air, without any affiftance from the remainder.

Secondly, If thefe pofitions are juft, it muft happen, that if a cargo of corn be unequally circumftanced in relation to its permeability, the whole of the air difcharged by the pump will iffue through the *more* permeable parts of it, without affecting, in any degree, the *lefs* permeable ones.

Thirdly, In cargoes heated in any degree, and in thofe infected by that worm which faftens grains together by a web, the parts moft affected become much more clofe and denfely packed together than the reft, either by the fwelling of the heated grains, or by the web and dung of the worms which occupy the intervals between the grains.

If fo, the parts of a cargo which require the moft ventilation will receive the leaft; but, in all cafes, it feems likely that the air difcharged will not regularly permeate the whole of the cargo, but will pafs through the parts where the grain lies lighteft, and leave untouched thofe parts where it is moft clofely packed together.

Anfwer to the preceding Objections.

Though the holes appear numerous, they muft be fmall, left the corn gain admiffion; and many (efpecially of the uppermoft) will be nearly, if not totally, ftopped by the preffure of the grains upon them. Befides, the pipes which convey the air towards the centre are not meant to be fo fully perforated as thofe at and beyond it; and all may be ftill lefs fo, if in practice found neceffary. But as the quantity of air delivered by the forcing-pump within five feconds

of time is equal to the contents of fixty * feet of four-inch pipe within the firft minute, the air (notwithftanding the manifold perforations, obftructed as it is in meandering through a mafs fo nearly compacted as the bottom of the cargo muft neceffarily be by the preffure of the heap above) will undoubtedly reach to the end of the pipes, and confequently affect the cargo even there.

Be it farther obferved, that the flux of air compreffed into an half-inch ftream, in its paffage through the nofle, to enable it to overcome the refiftance of the fpiral fpring H, no fooner paffes the valve E, than it expands itfelf to the compafs of the pipe; by which expanfion, and extenfion (at the

* Thus calculated:

	Inches.	
60 feet	4	
× 12 inches	× 4	
Produces 720 inches as a multiplier.	16	the fquare of the diameter of the pipe.
	× ,7854	
	= 12,5664	or area of the circle.
	× 720	length of the pipe in inches.

Which, divided by 231)9047,8080(39,1679 gallons and decimal parts, the whole capacity of 60 feet of pipe.

2117

..388

1570

1848

221, &c.

Then, a fingle difcharge of the forcing-pump being eight gallons, five fuch difcharges amount to 40 gallons, which is more than equal to the contents of 6 feet of four-inch pipe.

And as on the larger fcale of ventilators the pipes need not exceed the fame diameter, the power of the air injected, when its egrefs is ftopt, will increafe fufficiently to force its way through webs, mats, and other obftructions, though impervious to the atmofpheric fluid, unaffifted by fuch mechanic aid.

fame

ſame time) forwards, its power becomes ſo weakened, that ſmall egreſs only will be made, till the pipes are filled with a fluid more denſe than atmoſpheric air, which will then, as is juſtly noticed, iſſue where it finds the leaſt obſtruction, unleſs attracted to the ſpot moſt heated.

Many circumſtances may cauſe one part of the cargo to be leſs permeable than the reſt; ſhould it prove ſo, the means readily offer for airing and purifying even this.

Suppoſe the hatches to be caulked down, and the hold made impervious to water; in ſuch caſe, the lapſe of air, under the obſtructions met with in its paſſage, could by no means keep pace with the influx from the forcing-pump; conſequently, if the holes in the deck, deſigned for its exit, be kept cloſe-ſtopped till the pumper feels reſiſtance, all the intervals of the cargo, be they ever ſo minute or irregular, muſt be occupied by freſh air, which, when permitted to eſcape, will carry off impurities with it. And thus, by ſtopping and opening ſuch vents repeatedly, no part of the cargo could miſs of purification. and this perhaps may be the beſt mode of adminiſtering it.

Prevention is better than a cure.

In a veſſel equipped with the apparatus deſcribed, the inattention muſt be great, if the corn be ſuffered to ſuſtain any injury at all. By an early uſe of it, perſpiration and damps will preſently be dried away; heating of courſe will be prevented; and even the production of the pernicious grub alluded to: for, be the nidus of its eggs ever ſo productive, their embryos will not vivify, without moiſture to ſuſtain them. Wherefore, it ſhould ſeem that the corn-merchant in future will have little to fear, ſave the influx of ſea-water; and even this (if in ſmall quantities) will, by the frequent uſe of the ventilator, gradually dry away.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

ROYAL SOCIETY OF LONDON.

ON the 30th of November laſt, the anniverſary meeting of the Society was held. Sir Godfrey Copley's gold medal, which is diſpoſed of yearly on this day to the moſt deſerving member, was awarded to the Rev. Mr. Hellings.

The receipts of the Society for the year were declared to have been 1736 *l.* and the diſburſements 1563 *l.*

The meetings of December the 5th and 12th were occupied principally in reading the concluſion of Dr. Herſchel's paper on the power of penetrating into ſpace by means of teleſcopes. According to the Doctor's obſervations, it is impoſſible with the naked eye to ſee any ſtar ſmaller than thoſe of the 8th, 9th, or 10th magnitude. With his 40 feet reflector he has diſcovered ſtars which muſt be three hundred thouſand times more diſtant than the neareſt fixed ſtar. Where are the bounds of creation!

At the latter meeting, an abſtract of a regiſter of the weather at Lyndon in Rutlandſhire, kept by Mr. Barker, was read; as was alſo a paper on annuities, reſpecting the contingencies of three lives, by Mr. Morgan.

December 19th. A paper, by Mr. Jordan, on the irides ſurrounding the ſun, moon, &c. was partly gone through, when the Society adjourned for the Chriſtmas holidays.

January 9th, the concluſion of Mr. Jordan's paper was read; as was alſo a paper by Mr. Anthony Carliſle, on ſome peculiarities in the arterial ſyſtem of certain animals.

On the 16th and 23d, a paper on ſound.

COMET.

C. Lalande has announced that, on the 26th of December laſt, his fellow-labourer Mechain diſcovered a comet near the ſtar ς of Serpentarius, which could be diſtinguiſhed by the

the naked eye. It had a tail of a degree, and was proceeding in a ſouthern direction.

SUBSTITUTE FOR CINCHONA.

C. Zannetini, a phyſician who attended the French army in Italy, has made ſome experiments, by which it appears that the flowers and ſeeds of the common nettle (*Urtica dioides* LINN.) may be employed in fever inſtead of cinchona. This ſubſtitute was attended with a ſucceſs beyond all expectation, in tertian and quartan malignant fevers. The nettle often produces a ſpeedier effect than bark, for it heats in a great degree, and, when the doſe is pretty ſtrong, occaſions a lethargic ſleep. The doſe muſt never exceed a dram, and is given in wine two or three times in the courſe of 24 hours. Zannetini found this medicine of great ſervice to guard againſt that total exhauſtion which forms the principal character of malignant fevers; and he recommends a ſlight infuſion of it in wine as an excellent preſervative for thoſe who reſide in marſhy and inſalubrious diſtricts. In employing the nettle in fever, Zannetini gives the ſame caution as ought to be obſerved in regard to cinchona, that is, that it muſt not be employed where there is an inclination to inflammation, or where a continued fever, ariſing from obſtructions, exiſts. This diſcovery is not unworthy the attention of phyſicians, and deſerves at leaſt to be farther inveſtigated, as a great deal would be ſaved if cinchona could be entirely diſpenſed with.

CURE FOR DISEASED ELMS.

C. Boucher, ſecretary to the Society of Emulation at Abbeville, has lately publiſhed a memoir on the diſeaſes which attack elms, and the method of curing them, from which the following is an extract:—"Elms are frequently attacked by ulcers, which at length deſtroy a great number of theſe valuable trees. Duhamel ſuppoſed that this malady might be aſcribed to a plethora of the ſap; and C. Boucher, by numerous experiments, has eſtabliſhed this fact, and diſcovered a remedy. He obſerved, that local ulcers never attack the tree on the north ſide, but almoſt always on that expoſed to the ſouth. The elms chiefly ſubject to it are thoſe planted in marſhy ground, and in the neighbourhood of rivers. The ulcer is generally at a little diſtance from the earth, ſeldom

more than five or six feet above it. This disease, arising from a superabundance of sap, differs from another disease of the elm described in the *Journal d'Histoire Naturelle** for the year 1789, in this circumstance, that the liquor, when exposed to the atmosphere, soon acquires the consistence of a gum, and has a very saccharine taste.

To cure the trees attacked, C. Boucher pierced each of them with an auger, which he inserted in the ulcer itself, and then fitted to the hole a tube which penetrated to the depth of 1·10 inches. Sound trees pierced in this manner yield no liquor; but those which are diseased give a more abundant quantity, in proportion to the serenity of the weather, and as the wound is more exposed to the south. This effect is suspended by stormy weather and high wind. He observed that at the end of two or three days the efflux of the sap ceased, and that the wound dried up and was healed.

This, therefore, is a simple and easy method of radically curing elms attacked with this disease. It is probable that the same process applied to other vegetables, and particularly to some fruit-trees, would produce a like effect. Pliny, Columella, and Palladius, mention the same means as having been employed by the ancients, but it has not been before practised for many years.

C. Boucher has completed his observations on the elm, and proved that this tree is not an exotic, as some authors have advanced. The study of the ancients has proved to him that it existed in Europe in the remotest periods, and very evident remains of it have been found in old mosses.

The analysis which C. Boucher made of the sap of the elm shewed that it contains a pretty large quantity of the acetite of pot-ash, a little of the acetite of lime, a certain quantity of vegetable matter or saccharine mucus, and a pretty large quantity of the muriat of lime. There exist in it, also, slight traces of the sulphat and muriat of pot-ash. This is nearly the same analysis as that before announced by C. Vauquelin.

NEW PNEUMATIC APPARATUS.

Mr. H. W. Pepys jun. has constructed a mercurial exhauster on the principle of the Torricellian vacuum. The

* No. 5 and 6.

inventor

inventor has promiſed to enable us to lay the plan of the apparatus, with ſome of the moſt intereſting of thoſe experiments which have yet been made with it, before our readers in a future number.

DEATHS.

On the 19th of December, C. Montucla, author of an excellent hiſtory of the mathematics. Two volumes only of the new edition of this work were publiſhed before the author's death, but fortunately all his corrections for the remainder were finiſhed.

At Paris, on the 1ſt of January, the celebrated naturaliſt Daubenton, the friend and fellow-labourer of Buffon. He was born at Montbard in Burgundy in the month of May 1716. He ſtudied medicine, and intended to follow that profeſſion in his own country; but Buffon, being appointed intendant of the king's garden in 1735, propoſed to Daubenton to reſide with him, to apply to natural hiſtory, and to aſſiſt him in the grand labours which he was then about to undertake. In 1740 the fate and taſte of Daubenton were determined for his whole life. More than half a century devoted to the formation of the cabinet of natural hiſtory, which in 1750 was only a plain drug-ſhop belonging to Geoffroy, which he arranged methodically and enriched with productions of every kind, has given him a diſtinguiſhed rank among naturaliſts. Being admitted into the Academy of Sciences in 1744, he never ceaſed to enrich the collection of its memoirs with various papers for nearly fifty years. The greater part of them contain new facts and ideas reſpecting the claſſification of ſhells, on the hippopotamus, the ſhrew-mouſe, bats, foſſil bones and teeth, the ſituation of the great occipital foramen in man and animals, rumination and the temperament of ſheep, a deſcription of ſeveral kinds of new animals, or animals little known. He was interred with great pomp on the 3d of January in the garden of plants. His funeral was attended by more than 500 perſons, and two orations were pronounced on the occaſion by C. Lacepede and C. Fourcroy.

INDEX TO VOL. V.

Chromates,

END OF THE FIFTH VOLUME.

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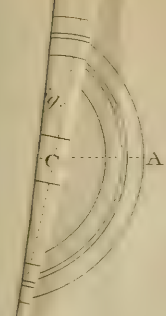

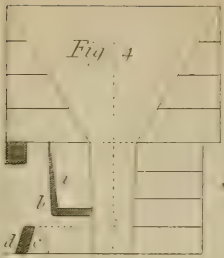

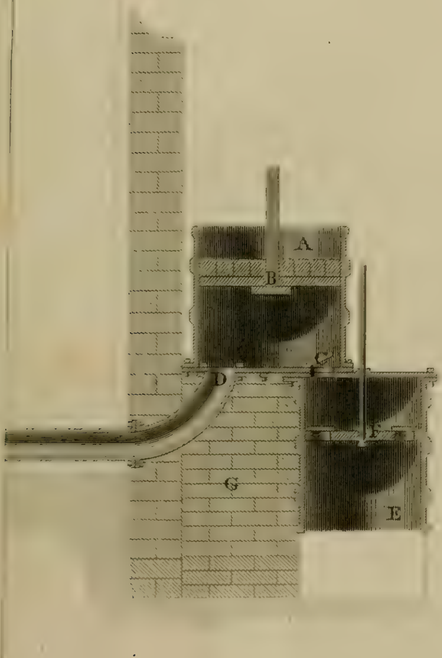

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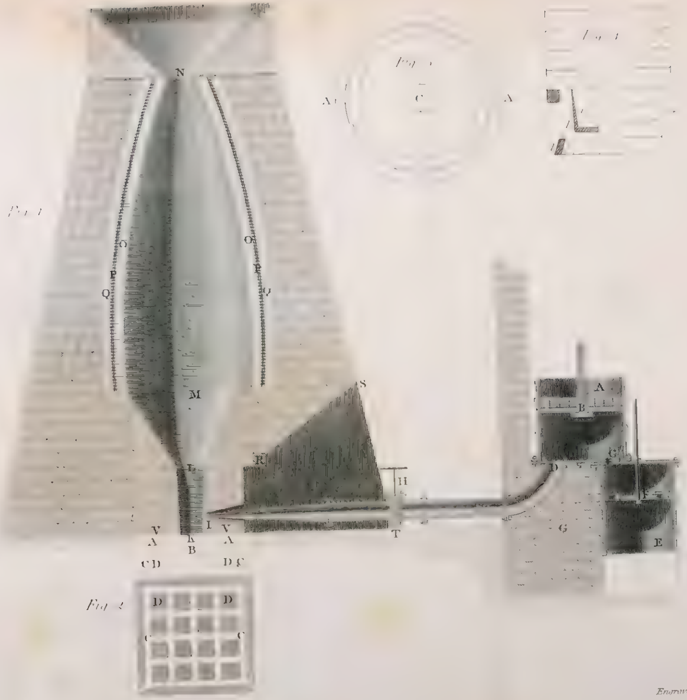
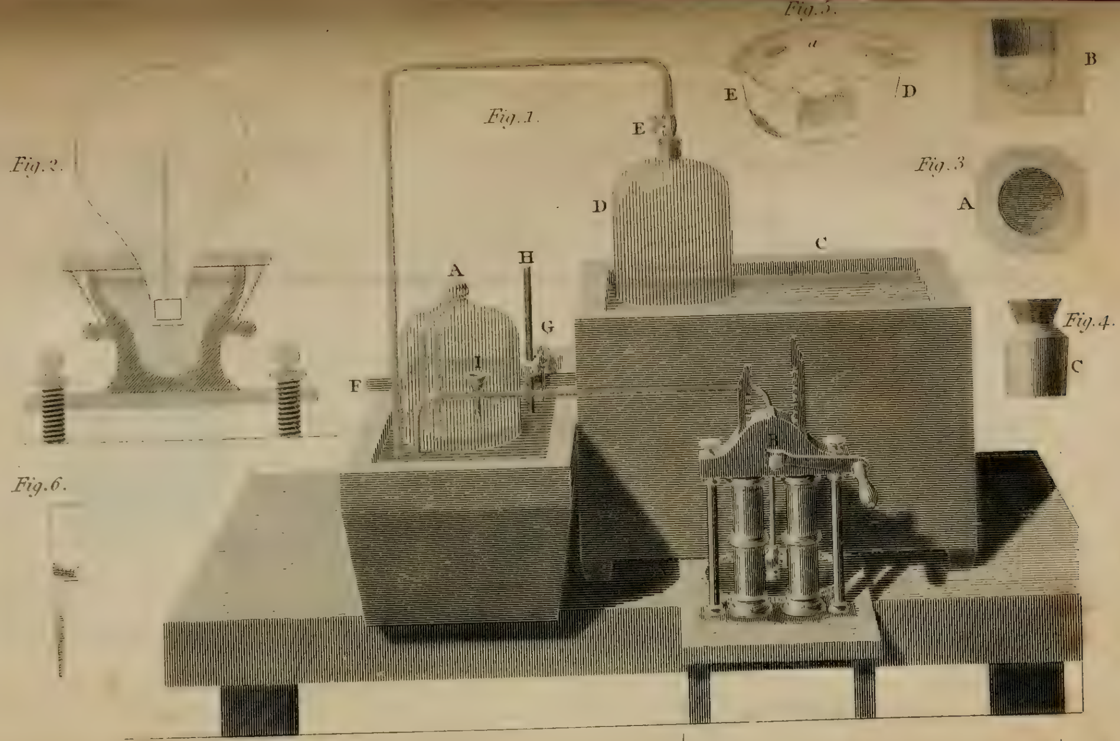


Drawn by D. Mushet

Engraved by Lowry



Lowry Sculp.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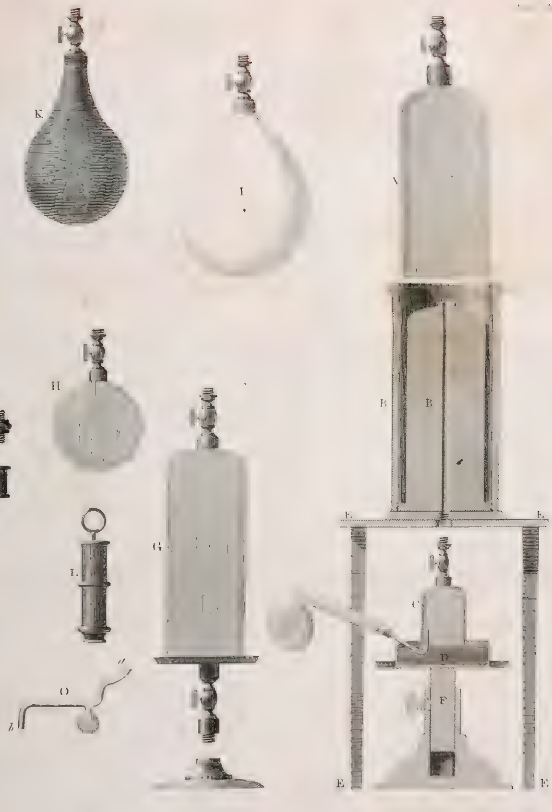
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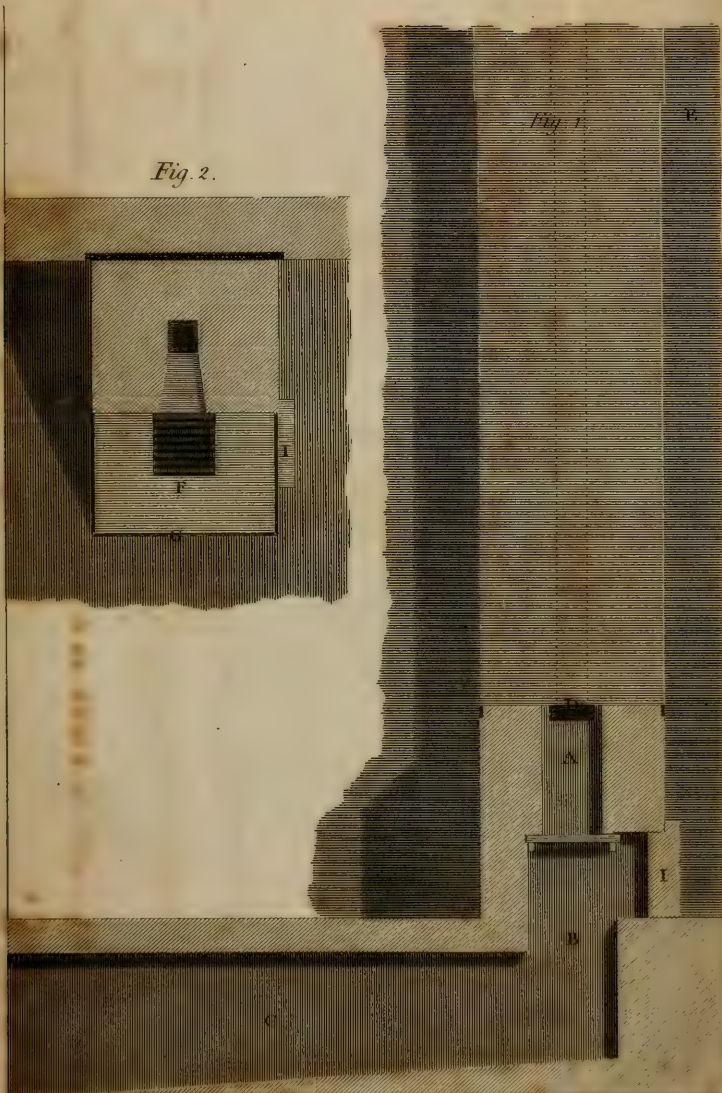
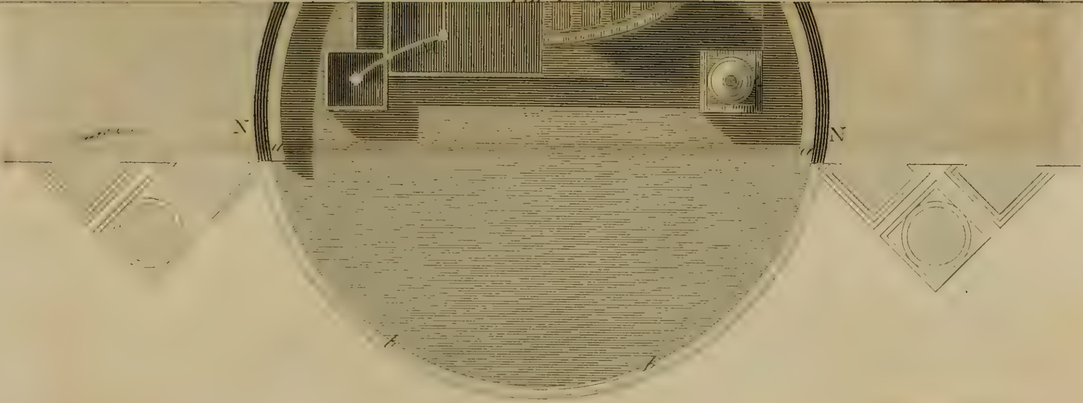


Lowry delin & sculp.

Fig. 1



Lowry Sculp

Fig. 1

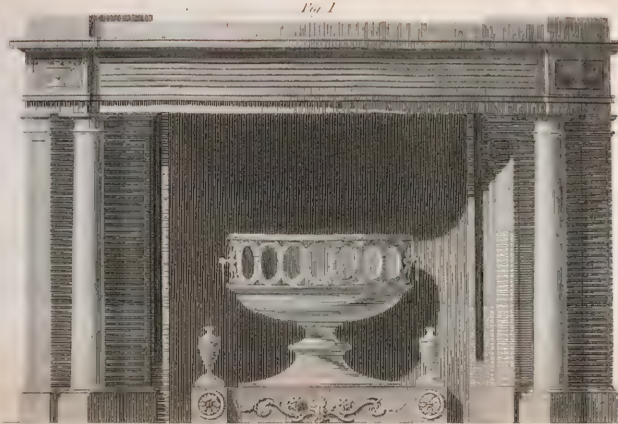
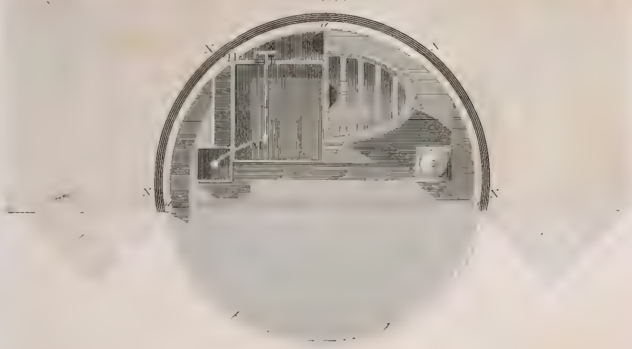


Fig. 2



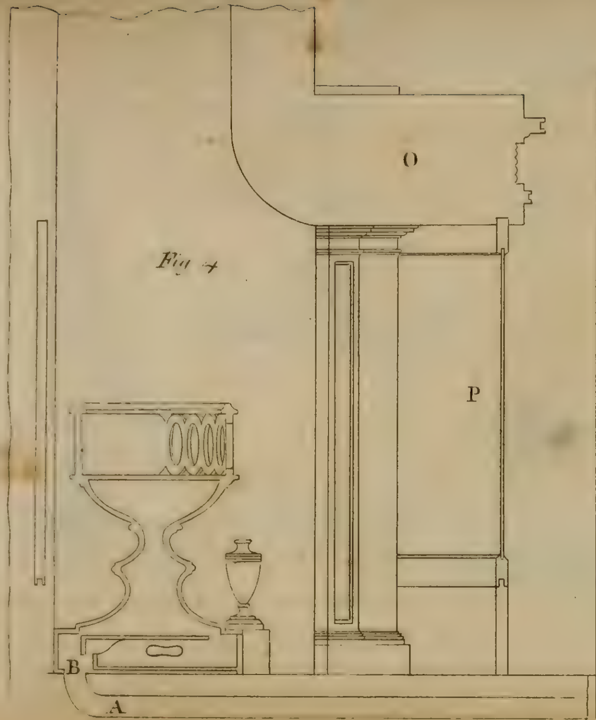


Fig. 4

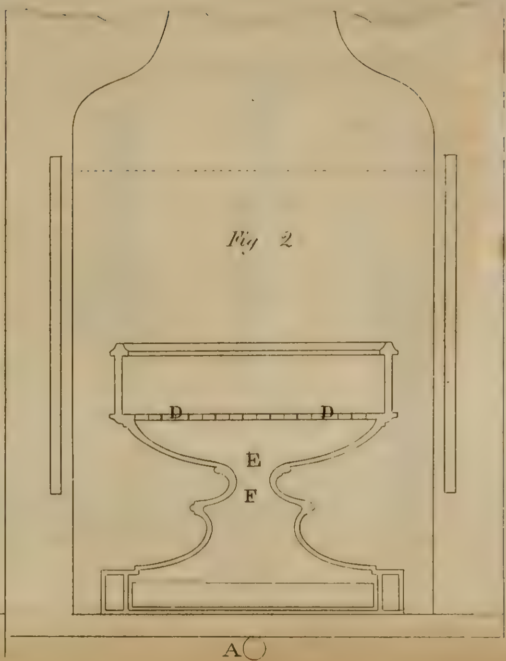


Fig. 2

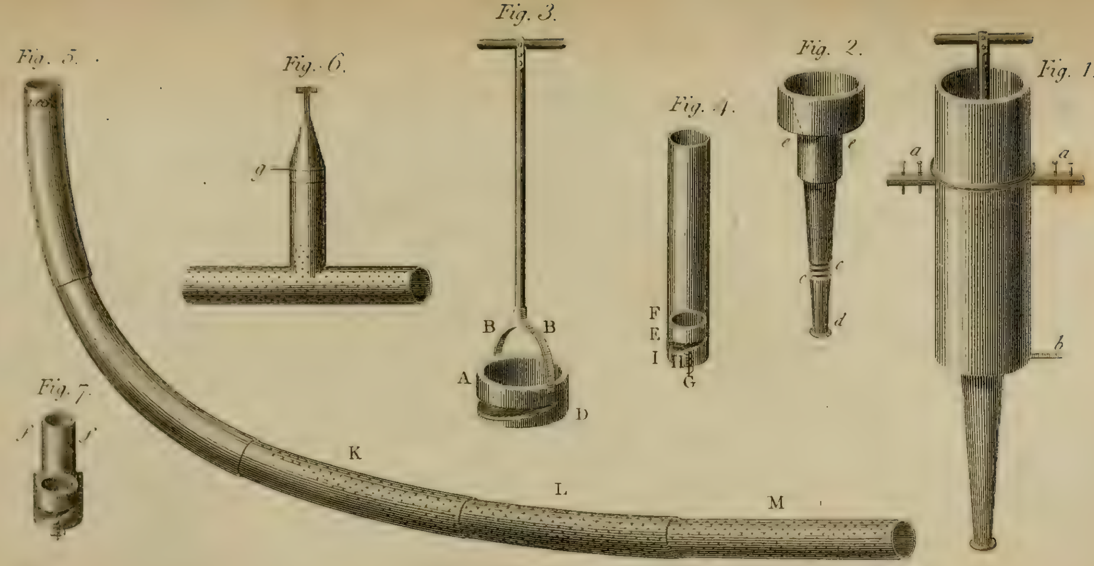


Fig. 1.
a
a
b
Fig. 2.
e
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c
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d
Fig. 3.
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Fig. 4.
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Fig. 5.
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Fig. 6.
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Fig. 7.
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Lowry sculp.

Baffins Bay
M
N
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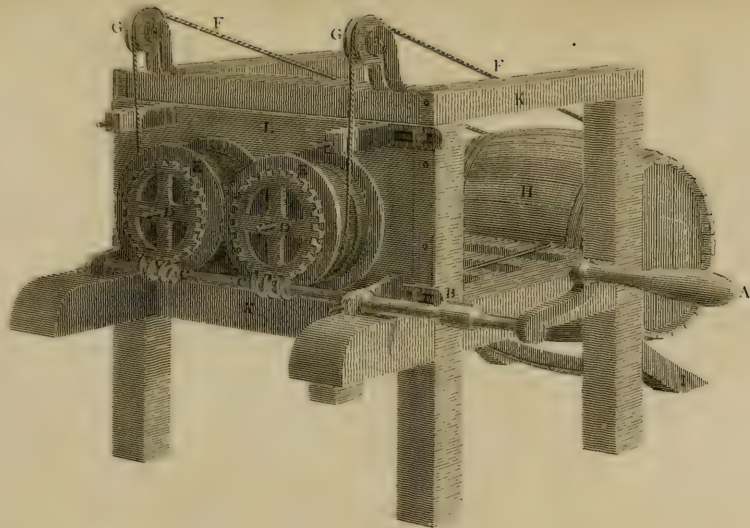
S
GREENLAND
WESTERN OCEAN
L
BALTIC
EUROPE
MEDITERRANEAN
AFRICA
Lowry sculp



Lowry sculp.